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REPORT OF COMMITTEE

ON

SOUTHERN AND SOLAR OBSERVATORIES

WASHINGTON, U. S. A.
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REPORT OF COMMITTEE ON SOUTHERN AND SOLAR OBSERVATORIES

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The undersigned, members of a Committee appointed to consider certain large projects in astronomy, have given close attention to that duty and herewith present the conclusions reached. Other matters still pending or requests from the Trustees may necessitate further communications which we shall take pleasure in presenting as occasion may arise.

I. General Recommendations.

We strongly urge the adoption of each of these projects submitted to us for examination, so far as this can be done consistently with other obligations which the Carnegie Institution may feel bound to assume. Our recommendations refer to definite programs of scientific work to be accomplished and not to the establishment of permanent institutions. Should these programs be carried to a successful issue, we are aware that in the meantime other demands for astronomical work quite as important and not less pressing may arise. The question whether the Carnegie Institution shall consider it advisable to take them up is one which we will not attempt to discuss at the present time.

For the present we urge the establishment of an observing station in the southern hemisphere for the accomplishment of certain definite works of observation, so arranged that their completion may be anticipated within a period of ten or twelve years from the time of beginning.

We also urge the establishment of an observing station for solar investigation in exceptionally favorable conditions of atmosphere, to be kept up during one full sun spot period at least (eleven years), or, preferably, through the maximum which may be expected to occur about 1916. In connection with this station (or with both stations) we recommend the construction and maintenance of a powerful reflecting telescope, as large as it is thought prudent to undertake, for use in astrophysical investigations upon the stars.

In appropriate sections of this report and in the appendices the several projects are discussed in some detail. We are indebted to our scientific colleagues for important advice and suggestions upon the matter herein treated, for which our best thanks are due.

These projects involve a large expenditure of money and scientific effort. Their importance demands that the reasons justifying an expenditure so great should be stated somewhat in detail, and that the character and importance of the scientific works for which provision is desired should be indicated.

Before proceeding to these details, however, it may be well to take up some of the general considerations that have a bearing on these projects.

Under its policy, as we understand it, the Carnegie Institution would not be inclined to establish an astronomical observatory for the general promotion of astronomy without more definitely expressed object. In lieu of special instructions the Committee looks for guidance to the declared policy of the Trustees as found in the Year Book for 1902 (pp. xxxvi–xxxvii). From this it appears that:

> "The Institution does not propose to undertake—
>
> (*a*) To do anything that is being well done by other agencies.
>
> (*c*) To enter the field of existing organizations that are properly equipped or are likely to be so equipped."

Aside from the support which it is the policy and practice of the Carnegie Institution to extend to investigations to be carried on through existing agencies, it desires

> "To promote original research by systematically sustaining—
>
> (*a*) Projects of broad scope that may lead to the discovery and utilization of new forces for the benefit of man.
>
> (*b*) Projects of minor scope that may fill gaps in knowledge of particular things or restricted fields of research."

These declarations seem to mean that the Instituution desires especially to assist in repairing notable deficiencies in knowledge. There is a kind of balance in the progress of various branches of research which it is desirable to maintain, so that results in one line of inquiry shall not remain too long unused in the archives of science while another line of investigation is bringing what is necessary for their proper interpretation. A virtual deficiency in knowledge may also be created through the neglect of opportunity to undertake some promising line of investigation, caused either by the forbidding cost of preparation or by some special inconvenience attached to its prosecution.

It will not be difficult to show, under this interpretation of the policy of the Trustees, that the projects considered here are singularly adapted to the support of the Institution.

Drawbacks to be considered.—It is proper, however, that we should call attention to one of the drawbacks which might result from the establishment of either of these observatories upon a temporary basis. The withdrawal from their present relations of a large number of competent astronomers for temporary duty with these observatories must be productive of some degree of disorganization in those institutions from which they may be recruited. The return of these men to more permanent relations may also be attended with more or less uncertainty, in view of which it is obvious that the compensation of the astronomers required for temporary duty must be decidedly larger than would be necessary under other conditions.

Another disadvantage of a temporary as compared with a permanent organization is the large proportion which the expense of installation bears to the total expenditure involved, together with the fact that the termination of the programs proposed would leave this expensive equipment idle on the hands of the Institution, unless some further provision should be made for its useful employment. This further use, however, could be effected in either of two ways:

(1) By provision for further employment on the part of the Institution.

(2) By donation or sale of the instruments to existing institutions which are in a position to make good use of them.

Relation to existing institutions.—The Committee desires also to record, in the most explicit manner, its opinion that nothing which may be determined in relation to large projects in science should be allowed to impair the total amount of support which the Institution

now extends in aid of astronomical research at existing institutions, so long as the results obtained shall show that such aid is efficiently expended. By the encouragement of several active centers of astronomical activity the amount of valuable astronomical product is stimulated out of all proportion to the means expended, and the future development of a number of able astronomers is made more certain than would be the case in a policy of greater centralization.

Proposed Observing Station in the Southern Hemisphere.

The project for a Southern Observatory was advocated in the general report of the Advisory Committee on Astronomy for 1902. Among other things, the committee says:

"The third point which has specially impressed itself upon our attention is the great deficiency of observatories in the southern hemisphere. * * * Since more than one quarter of the entire celestial sphere is efficiently reached only from the southern hemisphere, it is obvious that there is now very great disparity of astronomical resources to the disadvantage of the southern hemisphere. * * * We regard this question to be exceeded in importance only by the urgent need of provision for current work to which we have already alluded." (*Year Book* for 1902, pp. 89–90.)

The matter was further discussed in Appendix A and elsewhere in the reports of the committee.

We advocate the establishment of another active astronomical station in the southern hemisphere because there is needed in certain special lines a much greater output of astronomical observations, which can be obtained only by means of an observatory in some part of the southern hemisphere. This need veritably exists, as we shall attempt to show. It has grown out of the present progress of astronomy. The satisfaction of this need seems possible only through the aid of the Carnegie Institution.

What, then, is this need? We attempt to answer this question more fully in the section of this report devoted to special consideration of the proposed Southern Observatory; but we may be permitted to touch upon it briefly in another way here.

For three centuries astronomy has been developing by a rapidly increasing ratio of progression in attack upon the most accessible sources of knowledge. Until after the middle of the nineteenth century it was almost wholly absorbed in

(*a*) The study of the shape and dimensions of the earth and other terrestrial problems;

(*b*) An investigation upon the motions of the bodies associated together in the solar system.

The problems encountered in these two lines of research are of singular fascination, and they are capable of development in essential points upon the basis of observations that we now regard as comparatively crude. The element of time was favorable. Observations extending over relatively few years are sufficient to develop the facts of planetary astronomy in their broad outlines. It was natural that these subjects should have received the first attention of astronomers. The number of separate objects concerned was small, so that the work of observation did not bear a relatively large proportion to that of mathematical development of them.

But the time came, during the last third of the nineteenth century, when astronomers began to feel satiated with their conquests in the terrestrial and planetary fields. Facts and observations relative to the astronomy of the stars had been slowly accumulating. Plans were made and carried out to increase the extent and value of these observations. The *non possumus* of the more conservative astronomers of the old school fell upon less and less willing ears. The influence of the ideas which led to the establishment of the great Pulkova Observatory, sixty years ago, began to be more and more felt. On all sides it is perceived that the *sidereal problem* is to be the astronomical problem of the twentieth century, as the planetary problem was the characteristic problem of the eighteenth century.

Thus a great movement has been inaugurated for the accumulation of facts and observations in sidereal astronomy. The scope and meaning of the movement may be learned from a brief review of a part of the work accomplished or in progress :

(1) We are witnessing now the greatest activity ever known in the history of astronomy for the accurate, systematic, and extensive measurement of the positions of the principal fixed stars at successive epochs.

(2) We have seen the plan of the *Astronomische Gesellschaft*, for the accurate observation of all stars down to the ninth magnitude in the northern heavens, brought to the point where its success is fully assured. This is by far the most extensive work in astronomy ever attempted, previous to the project for the Astrographic Chart next to be mentioned.

(3) We are now watching the successful prosecution of observations for the Astrographic Chart—the attempt to measure from photographs the accurate positions of all stars down to the eleventh,

and to chart all down to the fourteenth magnitude. This gigantic project has been taken up with a degree of faith and a force of determination which proves the widespread interest that is now felt in sidereal problems.

(4) There is remarkable growth in the number of successful attempts to measure the motions of stars in the line of sight. Twenty years ago it seemed almost impossible to make these measurements with sufficient accuracy; now, at a few observatories it has become almost a matter of routine; and seven or eight of the largest telescopes in the world are dovoted in part to this work.

(5) Until comparatively recent years the measurement of the distances of stars was taken up here and there; but rarely did a single astronomer attempt to make this measurement for more than two or three stars. Now this work is carried on successfully for series of stars by observers who are devoting many years to it.

All of these works, and many others not here enumerated, have been undertaken for the purpose of throwing light upon the astronomy of the stars, in and for itself.

But activity in these and similar lines is almost wholly confined to the northern heavens. The resources required for extending these researches over the southern sky are wanting, except for the Astrographic Chart. Even for that it is a question whether the present or prospective resources of the southern hemisphere will prove sufficient.

Certainly there is no one subject in physical science that seems better entitled to command some part of the interest of every intelligent man than that which relates to the structure and mechanism of the vast aggregation of stars and nebulæ which challenge the curiosity of all beholders. The scale upon which the visible universe is constructed, and the inconceivably rapid as well as perplexing motions which prevail among the bodies that it contains, propose to our minds problems which have a high degree of interest, both physical and philosophical. The nature of these problems we will touch upon more fully in a subsequent section of this report; and we shall there endeavor to show that there are problems which justly concern astronomers of the present generation. There are some secrets of Nature which may forever remain concealed from the eye of research. We do not know. But problems which concern geometrical relations and motions can be solved when time and opportunity are propitious. From what has been already learned about the structure and mechanism of the universe of stars, it is easy to see that very much more must be very nearly in sight.

Now, precisely the greatest obstacle to a clear view of the stellar problem is the comparative lack of information about the stars in the far southern sky that are invisible to northern observers. When a sufficient number of accurately observed facts concerning these shall have been obtained, research upon the sidereal problem will have received a strong impulse forward. If for any reasons it is desirable to know the accurate distance between two points, no expenditure of labor and skill upon the measurement of three quarters of that distance is of full effect if our knowledge of the remaining fourth depends upon a rough reconnoissance. The observational situation in the southern hemisphere is fairly illustrated by means of this comparison, which also illustrates how the execution of the proposed observations in the southern hemisphere would increase the value of what has been already accomplished by northern observers.

In the directions where we were most likely to obtain trustworthy information as to the probabilities we have made careful inquiry to ascertain whether there is any present likelihood of increase in astronomical activity in the southern hemisphere through existing agencies, and we have not only failed to learn of any such likelihood, but, as will be shown, it has become clear that the present and prospective resources of the southern hemisphere are scarcely adequate to the fulfilment of existing obligations, for which we had supposed that adequate provision had already been made.

The works of observation that we propose could probably be brought to a successful conclusion within ten or twelve years from the time of beginning. While we are confident that the necessity for maintaining the proposed observing station for a still longer period after the expiration of its first mission would be as keenly felt then as now, that will be a question for the future to decide in the light of experience. Funds of the Institution need not be tied up for this purpose in perpetuity.

Thus we have in the proposed Southern Observatory two important requisites that would seem to commend it to the favorable attention of the Carnegie Institution:

(*a*) A project of broad scope, embracing also features of minor scope, that may lead to important discoveries and that will certainly "fill gaps in knowledge of particular things."

(*b*) It does not "enter the field of existing organizations," and does not propose "to do anything which is being well done by other agencies."

In the section of this report which deals with the proposed Southern Observatory various definite works are enumerated with particular care, and reasons are given in each case why it is important that they should be executed at this time. An effort has been made to indicate the bearing of these works upon the current of astronomical research, proving the manifold benefits which are certain to accrue to a numerous class of investigations. Should this scheme be carried out to a successful conclusion, it will hereafter be found that the results will become interwoven with the progress of knowledge. The points of contact and of essential support will become numerous beyond the power of present estimation.

What are the chances of success in carrying out the project should it be undertaken? This is certainly a pertinent question. We shall not attempt any over confident prediction in this direction. Much depends in a matter of this kind on the manner in which the enterprise is conducted, and also upon the point whether the means are secure and proportioned to the end; but we are able to say with confidence that there would be few obstacles in the way of success. The importance of the various works is incontestible; this should inspire those who are to execute them. The observers need not be troubled by the least suspicion of the futility of what they are doing. The success of the work does not depend upon the success of hypothetical experiments. It is not proposed to do anything by a process so novel that the guidance of experience in any material degree will be wanting. Even the probable duration of the works can be quite accurately gauged through the teachings of abundant experience. From the outset, in all really essential features, the observers will know what they ought to do and how they ought to do it.

The questions of organization and choice of site for the proposed observatory in the southern hemisphere present some problems which call for careful thought. The project admits of various scales of expenditure, but it is earnestly to be hoped that the scale adopted by the Institution would be sufficient to cover all of the works recommended.

Proposed Observing Station for Solar Investigation.

Among the projects commended by the Advisory Committee on Astronomy in 1902 is that for the establishment of an observing station for solar research at an elevated point where the atmospheric conditions should be of exceptional excellence. Closely associated

with this project and equally commended to favorable attention was another project for the construction and maintenance of a large reflecting telescope to be used in stellar researches.

In the domain of astrophysics there appears to be no direction in which there is a more hopeful prospect of a marked advance in knowledge to follow a large investigation wisely planned.

It is not surprising that the nature of the sun should have been an object of speculation from the earliest times. In the estimation of mankind generally the sun seems to be obviously the most important of the celestial bodies. From the astronomer's point of view, however, the sun has another interest which is not less intense and not less impressive.

The sun is a star. It is the only star near enough to us to permit of detailed study into its physical constitution. The next nearest star of whose existence we are now aware is nearly three hundred thousand times more distant than the sun. It should not be difficult to understand that if we wish to study the physical condition and history of stars under such difficulties of distances, it would be an immense gain if we could attain detailed knowledge as to any one of them which might happen to be near. The sun offers this opportunity.

It is already known that a great variety of successive stages of development seem to be illustrated in the analysis of the spectra which the stars present. There appears to be no link missing from the hottest and brightest to the coldest and least luminous. In the study of this development, then, it is manifest that we should gain a great advantage—the key to the position—if we could thoroughly know the physical constitution and tendencies of one in this series of stars. The sun is the one which offers this opportunity.

But astronomers have sought to take advantage of this opportunity during three centuries. For the last fifty years an immense total of effort has been expended on this research. For more than forty years the spectroscope has been in the hands of astronomers as an effective weapon of solar research. There are numerous institutions where solar research is now actively carried on. Where, then, is the field which would justify a special effort on the part of the Carnegie Institution to enter upon solar research at the present time?

The expanding record of our knowledge relating to the physical constitution of the sun is parallel with that of increase in the power of the appliances used in this research.

First, we have the improvement of the telescope. In its application to this purpose, we believe that this improvement has by no means reached its limit.

Second, we have the development of spectrum analysis—the study of light radiations in the spectrum. Rapid improvement in the apparatus which this method employs has been continuous for nearly forty years, and seems to be more strongly marked now than ever before. This is well illustrated in the recent development of the spectograph and the spectroheliograph, and in the marked advance brought about in the size and quality of diffraction gratings.

Third, there has been developed within the last quarter of a century the power of accurately investigating the distribution of heat radiations in the spectrum of celestial objects, and especially in that of the sun. An immense advance in knowledge has been recorded in this way through the accurate representation of a large portion of the solar spectrum containing rays invisible to the eye and beyond the reach of the photograph.

These facts alone might not be sufficient to fully warrant the project of combining in one large establishment the harmonious cooperation of these three methods of research developed to the highest state of science at the present day, but they should certainly be sufficient to excite interest in the question.

The present state of our knowledge of the sun is surprisingly deficient in comparison with that which would seem to be attainable by the use of the most powerful appliances. The fact that these appliances have not been brought to bear in anything like the extent demanded by needs well understood affords no very hopeful outlook in the immediate future that these needs will be met at existing institutions. What immense additions to knowledge are possible through the intelligent invention and use of powerful instruments of research in this line is well illustrated in the normal solar spectrum produced by Rowland less than twenty years ago, by methods remarkable for their simplicity, which at once superseded all the laborious efforts in that direction resulting from the use of less powerful apparatus during the preceding thirty years. We believe that similar opportunities for progress in solar research are within the reach of investigators of the present day through the provision of apparatus of which the possibilities are now generally conceded.

A distinct advantage might also result in the attempt to carry on in one institution the three related methods of investigation—tele-

scopic, either visual or photographic; spectroscopic, involving spectrum analysis in its various forms; and bolometric analysis of heat radiation. These may be likened to three senses by which solar phenomena can be apprehended and described. One of these senses may become aware of some passing and suggestive phenomenon in regard to which the evidence of the other senses will be immediately required. There would appear to be a strong likelihood that a great gain will be experienced in the study of local developments on the surface of the sun simultaneously from three points of view.

The chief obstacle to progress, however, appears to reside in atmospheric disturbances which prevent the full advantages that otherwise might be realized from the use of powerful optical appliances. There is scarcely any department of astronomical research wherein this difficuly is more acutely felt. Very naturally nearly all astronomical institutions founded by governments and universities have been located in or near capital cities or other great centers of population, where the manifest advantages and stimulus of a scientific atmosphere may be obtained. There is doubtless the desire also to maintain the visible connection between them and the power by which they were created. The observatories of Paris and Berlin are in the heart of great cities. In the case of very few existing observatories can it be said that the choice of site was influenced in any marked degree by a consideration of superior advantages in atmospheric conditions.

But superior atmospheric conditions are precisely what solar research most requires. Where these do not exist, small telescopes frequently answer almost as well as large ones. The air is in an almost continuous state of agitation, which prevents the employment of high optical power. Dust and smoke absorb a portion of the solar radiations, and these in an unequal degree. If we are to establish a solar observatory in ideal conditions, we must seek to avoid these disadvantages. While we shall not be able to accomplish this perfectly, we ought to inquire what gain may be fairly within reach.

Although the Carnegie Institution is free to locate a solar observatory anywhere in the world where this can be done to the best advantage, we have still thought it likely that the proper balance of advantages can be best secured within the United States. For some purposes a subtropical station might offer peculiar advantages for certain sections of this research. If the observatory had but one thing to do, and if the methods of observation could be formu-

lated and reduced to routine in advance, the arguments in favor of a subtropical station might be stronger than they are; but we know of no instance of the development of an important research outside the temperate zones. One of the highest obligations of the proposed observatory would be to follow up and take advantage of the indications brought out by successive programs of observations. Interpretation of results and choice of new and promising lines of attack would probably be the yearly experience of such an observatory, even though the main current of its work would consist of comparative observations maintained through a long period.

In order to secure the greatest freedom from atmospheric disturbances and from the absorption of solar radiations by the atmosphere, one might suppose that an extremely high elevation, where the observer would have below him as much of the atmosphere as possible, should be chosen. How great this elevation ought to be is involved in more than one consideration. The cost of maintenance would always increase very rapidly with increasing altitude. This becomes an important obstacle where a large observatory is in question. At elevations much above 10,000 feet extremes of temperature are likely to prevail, hurtful alike to the action of the instrument and to the observer. The rarity of the atmosphere in producing mental enfeeblement and prostration is a most serious drawback at extremely great elevations. No doubt men accustomed to sea level conditions can also live at altitudes exceeding 15,000 feet, if they have nothing in particular to do; but if they are to be called upon for prolonged and intelligent exertion, the case is different. Furthermore, it is quite unlikely that the most favorable atmospheric conditions are to be found above a certain moderate elevation.

These considerations moved us to seek an elevation of from 4,000 to 6,000 feet for the main observatory, with the idea of selecting a suitable auxiliary station at a much higher elevation for certain observations not requiring great steadiness of atmosphere, but needing a transparent atmosphere.

Our preliminary examination of climatic conditions seemed to indicate that a suitable station could be found in California. The steps which led to the provisional selection of Mount Wilson, a few miles east of Los Angeles, as the main station for the proposed solar observatory are detailed in the section of this report appropriate to that subject, and in the report of Mr. W. J. Hussey (Appendix A), who was employed to make the requisite explorations and atmos-

pheric tests. The atmospheric conditions on Mount Wilson at an altitude of 6,000 feet, as reported by Mr. Hussey, appear to be remarkably fine, and this opinion is supported by Messrs. Campbell and Hale, who took part in the final tests.

Auxiliary station for solar observations.—Among the problems of the proposed observatory would be that of determining what is the total amount of heat radiated by the sun and to what extent, if any, this amount varies—the determination of the so-called solar constant. For this purpose it would be ideal to make this measurement from a point which receives the radiation of the sun without any absorption whatever by the earth's atmosphere. As an approximation to this, one might choose a station upon the top of the loftiest mountain which can be ascended; but for reasons already stated we are not in favor of this extreme elevation.

It has been thought that some such site as Mount Whitney, about 170 miles north of Mount Wilson, might offer a suitable auxiliary station for the special observation of heat radiations in relation to the solar constant. The altitude of this mountain is nearly 15,000 feet. This is an elevation beyond which it can scarcely be supposed that continuous observations could be effectively carried on. In 1881 Professor Langley occupied this peak for a similar purpose. We think it might ultimately prove that after occupation for two or three seasons such a relation with the results obtained on Mount Wilson might be established as to render the continued maintenance of such an auxiliary station unnecessary. The principal study of the heat radiations of the sun would then be made at the permanent station on Mount Wilson, or wherever the main station might be located.

In the section of this report devoted to the proposed Solar Observatory will be found a presentation of reasons for our conviction that this institution should be established, together with a carefully prepared description of the appliances that would be required.

Advantages of a great reflector.—As connected with both the Southern and the Solar Observatory, and a consistent part of them, we think that a large reflector should be mounted and maintained for the investigation of stellar spectra. From what we have already stated, it will be seen that we regard the relation of solar physics to stellar physics the most important inducement for the establishment of the Solar Observatory, though not the only one. In order to make a comparative study effective, there would be a great advan-

tage in associating the two branches of study, solar and stellar, in one establishment. But, aside from this, we think that the site which has been provisionally selected (or a still better one if it can be found) would be most admirably suited as the location for the most powerful telescope that can be constructed. The occupancy of this site for such a purpose seems to offer a rare opportunity that ought not to be neglected.

Furthermore, it is now conceded that the construction and successful operation of a reflecting telescope, having a mirror five feet in diameter, is entirely feasible. We have the benefit of experience, both at the Lick and Yerkes observatories, in demonstrating the great advantages to certain researches in astronomy which may be derived from the use of large reflectors. We feel no hesitation in offering the prediction that researches in astrophysics would be enriched to a remarkable degree by the use of the largest reflectors that can be made, and we would strongly urge the advisability of providing a very large reflecting telescope as part of the equipment of the proposed observatories.

Policy of proposed Solar Observatory.—Plans and estimates for a Solar Observatory, and likewise for a Southern Observatory, have been prepared upon our understanding that the Carnegie Institution is not in favor of establishing permanent observatories, but that works undertaken must be in the nature of expeditions for the solution of definite problems, in limited periods of time. In some ways this policy seems to us to have advantages, especially in relation to problems for whose solutions the instruments are of standard form and for which the methods have been perfected and thoroughly tested. Examples of this are meridian circle, heliometer, and micrometric work generally. On the other hand, this policy is not a natural one for those new works which call for developments in instruments and methods. Some portions of the solar work and the application of photography to parallax problems are examples of this.

The task proposed for the Solar Observatory is only in part of a routine character. A large part of its work ought to be one of development, in relation to which it would be difficult to foresee, in all its details, the exact character of the equipment which might be best calculated to meet successive requirements of investigation as they may arise. Consequently, we are of opinion that the director of the proposed observatory should not be hampered with a rigid program of method and equipment, prescribed in advance.

Problem of Organization.

It seems almost superfluous to add that the prompt equipment and organization of two observatories such as we have recommended might be a temporary strain of some severity upon the existing resources of astronomy. The only remedy for this which we can suggest is the avoidance of undue haste. There can be no question that the success of these enterprises would depend very largely upon the ability and energy of the agents selected to carry them out. It may safely be said that no enterprise in science is so important that it cannot afford to await the appearance of scientific men of sufficient foresight to appreciate its importance and of sufficient ability to carry such an enterprise to fruition. On the other hand, we think that it would be far more logical for the Institution to determine from the consensus of astronomical opinion what is best to be done in astronomy and then to take means to get that thing done, than it would be to build and equip an observatory with instruments and staff in the hope that such staff might find something profitable to do. In the former case it is but a question of time in finding the astronomer, previous to which no expenditure is lost; in the latter the expenditure is incurred at the outset and remains to be justified. The latter course is unavoidable in the case of governments and institutions which establish permanent observatories, and on the average it produces good results; but the Carnegie Institution enjoys the rare privilege of taking the former course, which we believe to be the safest where it can be employed.

II. Proposed Observatory in the Southern Hemisphere.

In inviting the attention of the Trustees to the project for an observing station in the southern hemisphere, we are not proposing a mere generality. We propose certain definite things to be accomplished.

This undertaking essays the humbler task of producing the facts of observation by means of which future hypotheses must be tested and upon which theories must be founded, rather than the more brilliant role of attempting some great scientific generalization; yet it does not altogether lack the attractiveness which belongs to great discoveries, since it is intended to prepare a road by which discoveries can be reached. In any future division of honors it cannot fail to win its share. In fact, it is the more sure of this because theories often fail and are replaced by more perfect deductions; but the observations always remain, partakers in final success. The feasibility of the works here proposed is undoubted. Their value can be safely predicted. This value will be enduring, not temporary.

But this project does not propose to build for posterity alone. It promises to lead to generalizations of immediate importance, and if the valuable deductions to which it may be fairly expected to contribute at once should fall short of expectation, it could only be a question of a few years before these could be realized.

The enterprise herein proposed offers a peculiar attraction in the powerful alliances of which it would form a part. It would form one of the converging lines which must result in producing what will prove to be the characteristic advance of astronomy in this century.

It will be natural first to consider the nature of the general problem in astronomy to the solution of which the establishment of the proposed observing station in the southern hemisphere may be expected to contribute.

Problem of the Sidereal System.

The mere appearance of the starry sky at night cannot fail to impress the reflecting mind with the thought that this vast aggregation of stars must contain within itself the evidence of organic arrangement. A more attentive examination, even without a telescope, while it reveals a general uniformity in the distribution of the stars

of various orders of brightness, also makes us aware that there are clusters of stars which point unmistakably to the operation of law. The vast aggregation of clusters making up the nebulous belt known as the Milky Way, which spans the sky as a kind of celestial equator, suggests orderly arrangement.

When we assist our researches with the telescope, merely as an optical appliance, evidences of such arrangement multiply until we are finally led to conjecture, by statistical methods alone, that the earth is situated within a vast cluster of stars very much more extended in the direction of the Milky Way than in other directions.

If we fortify our telescope with means of measurement we shall discover, after sufficient lapse of time, that some of the stars are in motion relatively to others. As we persevere in our measurements we shall discover an ever increasing number of stars partaking of this motion ; and we shall finally conclude that all the stars are in motion, some in one direction, and others in another. Some of these motions are only apparent. Disentangling these, after immense effort, we shall be able to recognize a peculiar drift of the stars, precisely like that which appears in surrounding objects when we are moving rapidly among them. This will finally prove to us that the sun is only a star, and that it is in rapid motion like all the others. Later we shall notice that separate groups of stars seem to be moving in a common direction like swarms of meteors ; and we shall begin to suspect that other evidences of law in these motions may be revealed to us at any moment if we persevere in our investigations.

Parallel with the discovery of these facts we shall be learning something of the distances of the stars. We are oppressed with the conviction that, so long as we are studying the arrangement and motion of the stars as objects upon a map, our information will be lacking in a vital element. We long to gain some conception of the space relations of the stars. From the very first we will have been led to suspect that some stars are brighter than others because they are nearer. When we attempt to test this hypothesis by measurement, we shall encounter the most formidable difficulties. Putting this difficulty in its simplest form it will mean that, if the vast area of the earth's orbit, more than one hundred and eighty million miles in diameter, were to be brightly illuminated and removed to the distance of the nearer stars it would appear as a mere point in the most powerful telescopes ; and to discover its dimensions would require the greatest refinement of skill even in the more favor-

able cases. Nevertheless, we shall be able to verify our conjecture in part, though we shall find that motion rather than brightness is the better criterion as to the distances of stars. Later we shall discover that we can test the relative distance of one large aggregation of stars from another, through one of the generalizations resulting from an investigation of the solar motion. Now, we shall have arrived at the point when we shall feel that we may some time see the stars in space of three dimensions—when we shall not only be able to make a flat picture of their relations as they appear to the eye upon the celestial vault, but shall also be able to construct a model showing the special and general relations in distance in space of three dimensions. Astronomy is standing at this point now. It takes some pride in what it has accomplished in this line already; but it is also more dissatisfied than ever with the amount of accurate knowledge which it has, because it sees how comparatively easy it would be to increase this knowledge in a very large ratio.

Again, parallel with these researches upon the motions and distances of the stars, there has been growing in importance another class of inquiries. Arming the telescope with a new appliance, the spectroscope, the evidence of universal motion among the stars, whether toward or away from the sun, has been most brilliantly confirmed. In this way the solar motion has been verified, and the approximate velocity of the sun in its flight through space has been determined.

Through its power of physical and chemical analysis the spectroscope has brought to light a collection of new facts of the highest importance in their bearing upon cosmical problems. It has shown the presence of terrestrial substances everywhere among the stars. It has demonstrated anew and more convincingly that the sun is a star, and even that there are multitudes of stars closely resembling it in their physical condition. It has shown us something of what is going on in the development of suns, or stars, revealing them at all stages of evolution and connecting one with another by imperceptible gradations from the newest to the oldest forms.

The unity of the stellar universe is thus demonstrated from three distinct points of view, and the conviction is brought home to us that there must be within our reach further and more interesting facts which will illustrate something of the arrangement and mechanism of this vast system of worlds flying hither and thither through space.

In outline this is what we shall term the *sidereal problem*, which

is now coming into view as the problem of the time, surpassing in the universality of its interest and in the grandeur of its scope all the physical inquiries which engage the attention of scientific men.

This problem should not be regarded, however, as synonymous with the whole range of sidereal astronomy. In the sense in which the term sidereal problem is employed here it would include those classes of research the chief interest of which centers in their bearing upon the description of the stellar system as a unit. There is another large range of extremely interesting investigations in the stellar field, in relation to which the chief interest centers in the natural history of individual objects. Even these may turn out to have their bearings upon the sidereal problem as a unit, but the connection is not so apparent. However much one class of researches may blend into another, the distinction here made may be found a useful one in the effort to give unity of design to the aim of the proposed Southern Observatory.

To unravel the problem of the solar system was the task which was first seriously proposed to astonomers three hundred years ago. Galileo, Kepler, Newton, and a long line of distinguished successors have successfully grappled with the intricate questions involved in that problem. Now the aspirations of astronomers are reaching beyond the boundaries of the solar system into stellar space. The stellar problem confronts us as the serious occupation of the present and the future, and it is illimitable in extent.

Need of More Astronomical Observers in the Southern Hemisphere.

Why should we go to the southern hemisphere in order to work upon this problem?

Were our object planetary research the inducement for another southern observatory might not be sufficient. All the planets can be seen from either hemisphere, and from either the whole fabric of planetary astronomy could be constructed without help from the other, though there would be undoubted advantage in cooperation.

But in stellar observation the case is different. Nearly one-quarter of the entire celestial sphere is inaccessible to exact observation from the observatories of the northern hemisphere. If we need observations of the nebulæ and stars in that part of the sky, we must go to the southern hemisphere to get them. For many of the most delicate researches fully one third of the sky should be under observation from the southern hemisphere.

Why cannot the observatories already in the southern hemisphere make all the observations that are needed there?

For this duty in the southern hemisphere we have about one tenth of the force which is available in the northern hemisphere for such observations—one tenth of the force to do one quarter or one third of the work. This point might be enlarged upon in detail, but it is so notoriously the fact as to make such illustration superfluous. The unanimous verdict of astonomers on this point may be derived from the correspondence which is transmitted with this report.

Nor does there appear to be any prospect now or within a reasonable time in the future that any material addition to the astronomical forces of the southern hemisphere can be anticipated through existing agencies. This ought not to be a matter of surprise. In addition to the numerous private, institutional, and municipal observatories in England, several astronomical establishments, including the celebrated Greenwich Observatory, are supported by the government in the British Islands. Therefore the British Government, in maintaining an astronomical observatory of high rank at the Cape of Good Hope, can scarcely be criticised for not doing more in that quarter of the world. Great Britain is the only one of the great powers from which support of astronomy in the southern hemisphere can be anticipated. Consider the local resources. We have the English colonies in Australia. Three small observatories are maintained there, of which, until recently, two have been almost exclusively concerned with meteorology. For somewhat more than forty years astronomical work has been carried on in the observatory at Melbourne, but the tendency recently has been to curtail this. Furthermore, the three Australian observatories have each recently undertaken a share in the Astrographic Chart, which is practically certain to absorb their entire energies for many years to come. Nor should the Australian governments be criticised because they maintain only three observatories on a small scale when we reflect that New South Wales, Victoria, and Western Australia combined have a population smaller than that of Texas. Still less should we anticipate any important contributions from the comparatively small communities in New Zealand, Tasmania, and South Africa. There remain only the governments of South America, and of these only two, Chile and Argentina, are in a geographical situation to become factors in consideration. Each of these countries maintains a national observatory. In the period from 1854 to 1862 the National Observatory of Chile, under a German astronomer, was active in a small

way. Recently a share in the Astrographic Chart was undertaken there, but Director Loewy informs us that this work has not been prosecuted there, and he expresses the hope that the Carnegie Institution may supply the vacancy. A strong national observatory was organized at Cordoba, in the Argentine Republic, in 1870, under the direction of Dr. B. A. Gould. This ranked among the leading observatories of the world for many years; but the financial disasters of Argentina have had a depressing effect upon that observatory, which, though it still continues its valuable work with perseverance and effect, is certainly in no position to undertake any additional obligations beyond that of a share in the Astrographic Chart and the other works there in progress.

Summing up, we find this situation: The Royal Observatory at the Cape of Good Hope will doubtless continue in its functions as an observatory of high rank, with the duty of doing for the southern hemisphere what Greenwich does for the northern as the primary object of its establishment. There planetary and stellar observations will be made, together with the observations that are required in the interest of government surveys. Over and above this we may fairly expect strong contributions from that observatory in a variety of fields. The remaining observatories of the southern hemisphere, with one exception, will be absorbed for at least 10 or 15 years in their work upon the Astrographic Chart, and, so far as can now be foreseen, will not be in a position to undertake much else of an important character. The one exception mentioned is the observatory at Arequipa. This observatory has been occupied in photometric and photographic researches of a nature similar to those carried on at Harvard College Observatory, of which it is a branch. These researches are of great interest and value, but they are outside the scope of the program here proposed.

Why Observations Are Needed in the Southern Sky.

It is desirable to consider more particularly, though briefly, why it is that the observation of objects in the far southern sky is so peculiarly desirable in connection with cosmical problems. If the problem were merely to ascertain whether the stars are in motion at all, or whether their motions vary with apparent brightness, or whether the distances of the stars are measurable in any case, these questions could be, as they have been, settled by observations on objects in the northern sky; but when we reach a point where

there is promise of fruitful generalization, the case may be different. It so happens that the various motions of the stars, real and apparent, are so involved that some means of separating their effects must be devised. Now, it happens that this separation is very difficult when the observations of one hemisphere are considered alone, while there is almost complete elimination of the interdependence of assumptions as to one class of motions upon those as to another when testimony is gathered from the entire sphere. To a very considerable extent this is true of the relation of precession to solar motion.

Again, different forms of a general rotation of the stars have been suggested. It is next to impossible to get any light on this question from the facts of observation relating to one hemisphere alone, because this effect is so entangled with the apparent motions caused by precession and solar motion that they cannot be accurately distinguished. With testimony from the entire sphere, a wrong supposition as to precession or solar motion would not so seriously affect the conclusion as to possible rotation, and in some circumstances scarcely at all.

Again consider the spectroscopic measurement of the velocity of the sun's motion through space. By combining the observations of both northern and southern stars we obtain the motion of approach contrasted with the motion of recession. If there should be anything in the suspicion that all the observations of velocity may be affected with some obscure source of error in common, this would be eliminated in the combination of the two hemispheres. In general it is obvious that we cannot neglect a large fraction of the sky when we are dealing with problems concerning the whole universe.

One consequence of this reasoning is that the proposed program for a southern observatory has this peculiar merit: that the results to be obtained are not only valuable in and for themselves, as constituting a needed increase in the sum of human knowledge, but they increase in a marked degree the value of the facts of observation already obtained in the northern hemisphere.

It does not seem at all surprising that our colleagues, with emphatic unanimity, should have expressed the opinion that additional astronomical effort at the present time can be expended to the best advantage in the southern hemisphere.

We have thus, here, an important research upon which further special investigations are needed, and in reference to which it ap-

pears very improbable that they will receive proper attention from other agencies within a reasonable time in the future.

Works of Observation Proposed and Instrumental Requirements.

We come now to the consideration of the particular works of observation which, in our opinion, could be undertaken to the best advantage by the proposed Southern Observatory. It is neither possible nor desirable that this one institution should undertake to make up all existing deficiencies in astronomical observation in the southern hemisphere. Other agencies have a responsibility in this field. New forces coming to the rescue ought to, and doubtless would, inspire the existing forces of astronomy in the southern hemisphere to still greater effort.

While not attempting to establish an inflexible criterion of selection for works of observation, we are of the opinion that the primary aim of the proposed Southern Observatory should be to make itself felt in the attack upon the sidereal problem as we have defined it—that it should seek to throw light upon the structure and mechanism of the stellar system as a unit. In other respects less attention need be given to those projects of observation that would suffer less by delay.

In preparing the schedule of proposed works we have been greatly assisted by the advice of astronomers whom we have consulted for this purpose. Their letters in response to our inquiries will be found in Appendix B, together with a preliminary statement of works proposed which forms the basis of the comments with which we have been favored. These comments have proved a most welcome assistance, and we have been strongly influenced by them in making up the schedule of proposed works.

(1) *Fundamental Meridian Observations.*

We regard meridian observations of precision, upon the brighter stars, to be of the first importance in any attempt to relieve the situation in the southern hemisphere. In this we are sustained by the nearly unanimous verdict of those whom we have consulted.

We are clear upon the proposition that the exact positions of about 6,000 stars (including all down to the seventh magnitude that are south of —20° of declination) should be determined by fundamental methods, both for the interest which this work commands as an inde-

pendent research and for the bearing it has on other works, including those here proposed.

In this line of work, as in nearly all other departments of astronomy, contributions from the southern hemisphere have always been deficient. With a few notable exceptions, there has been a lack of highly trained and experienced astronomers there. The recent efforts of the Cape Observatory in this line have been both skillful and energetic; but the Cape Observatory alone is unable to offset the numerous observatories in the northern hemisphere engaged in the precise observation of standard stars. The result is that the weight of our knowledge of the positions and motions of the standard stars in the northern sky is fully five times that for the far southern sky. A single corps of observers transferred from the northern hemisphere, where its loss would not be a relatively serious matter, to the southern hemisphere, where its services are so much needed in this line, could reduce the existing discrepancy of weight in fundamental determinations for the present epoch by one half.

It has been conclusively shown that the exactness of our knowledge of general drift in the motions of the stars, whether it arises from solar motion, rotation, or from any other source, depends almost wholly upon the number and precision of our fundamental determinations of the positions of the standard stars. The main battle is fought on this field. Furthermore, astronomy has now arrived at the point where, by comparatively little additional observation, it will be possible to compute the motion of nearly every star brighter than the seventh magnitude in the northern sky with a fair degree of accuracy. With the observations herein proposed, the same thing, in a modified degree, would be true of the stars in the southern hemisphere. Not many more than one third of these stars has been reobserved during the last quarter of a century. This work accomplished, astronomers could hope to deal successfully with problems of motion for all the stars visible without a telescope, and beyond that for all stars down to those which are one third as bright as the faintest visible to the ordinary eye—about 15,000 stars in all. This would give us the first opportunity ever offered for a comprehensive discussion of the solar motion and related problems on a scientifically correct basis, with a liberal supply of material distributed over the entire sphere.

This work would, therefore, possess a high scientific interest as an end in itself; but it would also serve as an indispensable basis for the observation of planets and of the fainter stars. A scheme

of observation like that described further on under (2) would require this work as its foundation. In fact, there is scarcely a single department of precise measurement in astronomy that would not be indebted to this work for a part of the data which it needs.

The problem of observation here suggested naturally divides itself into two sections. The first would be concerned with the observation by fundamental methods, and with the highest precision, of about 600 to 800 of the principal stars; the second would involve the extension of this work by less rigorous methods to about 5,000 other stars—the entire work to be conducted in such a manner as to be systematically consistent in all its parts, and to be a homogeneous whole.

There can be scarcely a doubt that the ideally best result would be attained through the adoption of the transit and vertical circle, for the observation of the principal stars at least. This, however, would prove somewhat more costly in execution, and the completion of the entire work, if it should be carried through with these instruments alone, would be deferred somewhat longer than might seem desirable.

The advantage of economy would attach to the use of a meridian circle for these observations. This would be increased, if the use of such an instrument that has already been thoroughly tested in the northern hemisphere could be procured, and we believe it can. The labor of a thorough investigation of the errors of graduation and other errors of such an instrument is, in itself, no slight task. What would be requisite for the purpose, here designed, would probably be equivalent to one full year of work by four observers. The degree of accuracy really attainable in the use of the meridian circle should not be sensibly inferior to that for a vertical circle. The distinct advantage of the latter is in the variation of method which it offers. For the present we should advise the employment of a meridian circle for this research, unless the proposed Southern Observatory should be established on a scale which would enable it to maintain for certain lines of observation the highest ideals.

Whatever the precise methods of observation may be, the instrument, or instruments, employed should be used for at least two years in the northern hemisphere in the determination of the positions of the principal fundamental stars visible there. There would result a peculiar gain in precision through the comparison of observations with the same instrument upon the same stars, made alternately in the two hemispheres, by which certain errors of the instrument and

a part of the uncertainties in our knowledge of astronomical refraction could be in a marked degree eliminated. The proof of this is already at hand in numerous comparisons of observations made contemporaneously with different instruments in the two hemispheres. From these it can easily be inferred what would be the probable gain if both sets of observations were to be made with the same instrument. We think that this plan presents a unique opportunity which ought not to be neglected.

If the meridian circle should be employed with a full corps of assistants the southern work here proposed could probably be accomplished within four or five years from the date of beginning. It would involve about 40,000 observations of about 6,000 stars and about 4,000 observations of stars north of —20° for the purpose of comparison and check. From four to seven observers and computers would be needed, with as many more routine computers, in order to keep the work going continuously and the computations up to date.

(2) *Complete Observation of Stars to the Ninth Magnitude.*

The exact determination of the positions of telescopic stars down to the ninth magnitude and south of —30° of declination is a work of high importance. The opinions of astronomers would differ as to the pressing nature of this work in comparison with the two succeeding works mentioned in this schedule. As will be seen from the correspondence transmitted herewith, there are opinions of great weight equally positive in favor of each of these undertakings. The plan for meridian observation of faint stars is mentioned second in order because of its intimate logical connection with the investigation already mentioned under (1).

The project to observe with meridian instruments the precise positions of all stars down to the ninth magnitude originated somewhat more than thirty five years ago, and was adopted as the peculiar function of the *Astronomische Gesellschaft*, which at that time assumed something of an international character. The importance of this project has been universally recognized. The original program for the northern hemisphere was completed under a cooperative arrangement. The extension of this work under the same auspices has been carried to —22° of declination, and the observatory at Cordoba has pushed the work on to —32° of declination. About one quarter of the sky remains to be considered. The com-

pletion of this one quarter would greatly enhance the value of what has been already accomplished.

The demand for this work may be stated under three principal heads:

1. The primary utility of this work would rest upon its immense importance in studies relating to the stellar system. The work mentioned already under (1) would enable us to study the structure and motions of the sidereal system so far as this is represented by stars to the seventh magnitude. The extension of this work by means of (2) would take us to stars of the ninth magnitude, for which the older records of observations contained observed positions of the greater part. We should then be able to extend our studies upon the sidereal problem to stars ten times as numerous and more than six times fainter than those of the seventh magnitude. The feasibility of reaping the full fruits of this undertaking through the present generation of astronomers is not so great as for the brighter stars, but that which would be demonstrably attainable now in this direction fully warrants the enterprise. Furthermore, the completion of this monumental work, so that the accurate position of every star from the north to the south pole, down to the ninth magnitude, would be known for epochs near the beginning of the century, would be an achievement upon which the entire civilized world could look with pride.

2. The positions of these stars determined in this work would possess very great value as reference points in all micrometric work upon faint stars, nebulæ, comets, and planets observed by means of extra-meridian telescopes. This alone was originally looked upon as fully warranting the labor of the entire enterprise.

3. This work would furnish the reference stars needed in the work of the Astrographic Chart, which has for its aim the determination of accurate positions for all stars down to the eleventh magnitude. This forms the third step in a series of investigations which seek to determine the accurate positions of stars at successive epochs in order that we may ultimately learn their motions. The accuracy of which this third step is capable depends upon the accuracy of its basis, which must be the meridian observation of telescopic stars. The only difficulty is that the accuracy of the observations hitherto made under the program of the *Astronomische Gesellschaft* is not regarded as sufficient for the purposes of the Astographic Chart, as Director Loewy has pointed out. He recommends as a substitute for (2) the special determination of the star positions required for

the Astrographic Chart. It will be found, however, that the so-called zone observation of telescopic stars may easily be brought to a very much higher grade of accuracy than that which has prevailed hitherto, without any material sacrifice of the rapidity with which they can be made. This can be effected in three ways:

(*a*) By the use of a superior instrument.
(*b*) By the use of a more accurate and more extensive standard catalogue, rendered possible under (1).
(*c*) Through a better organization in methods of observation.

Then with the addition of about one fourth the number of observations which would be needed under (2), without reference to any ulterior use, we shall be able to complete the *Astronomische Gesellschaft* zones and at the same time meet the requirements of the Astrographic Chart; and we strongly recommend that this program be adopted.

The necessity of this extended program is all the more pressing because of the extremely doubtful probability that the observations for the basis of the Astrographic Chart can be secured by means of existing agencies with the requisite completeness and accuracy.

The argument that the positions of all the stars down to the ninth magnitude (and fainter) in the southern hemisphere can hereafter be derived from the Astrographic Chart assumes that we may look for the completion of that undertaking within a few years. The probability that the completion of certain important sections of that work may be delayed for a longer time than would be desirable, however, seems to be warranted by an examination of the situation. On the other hand, it seems very desirable that the great undertaking entered upon thirty five years ago for the determination by meridian observation of the position of all stars down to the ninth magnitude in the whole heavens should be completed.

The proposed Southern Observatory has here an opportunity to carry out a work which would have enough of intrinsic interest in and for itself, but which would possess the great added advantage of serving as the fundamental basis of another work even greater in extent than itself. It would also much increase the value of similar work already accomplished in the northern hemisphere. Furthermore, it would scarcely be possible to carry out the work proposed next in order (3) in an economical, effective, and comprehensive way, as to stars in sensible motion, without the completion of (2).

For this work there should be a corps of five or six observers and

computers and a somewhat larger staff of mere routine computers. Under these arrangements it should be possible in a good climate to turn out at least 20,000 accurate star positions each year, and to make all the computations necessary to put them in catalogue form. Since not more than 200,000 observations would be required, the entire work should be completed within ten years from beginning—possibly in less time.

(3) *Measurement of Stellar Parallax.*

The determination of the distance of individual stars is one of the severest tasks in the science of observation. The results already obtained during a period of more than sixty years, but chiefly during the last fifteen, are not free from troublesome discordances that seem to encourage pessimistic views. Yet a review of the work which has been accomplished, together with that of the larger amount which is now in progress, affords great encouragement that we shall be able to determine average distances of classes of stars with a very satisfactory degree of precision. This is the important thing we need to know in our first studies of the structure of the sidereal system. The two great questions to be solved are:

(*a*) What is the relation of brightness to distance? Are the stars of the sixth magnitude as far removed on the average from those of the second as the relation of brightness alone would lead us to think? According to this relation, the sixth magnitude stars should be on the average rather more than six times as far removed from us as are the stars of the second magnitude. Is this really the fact? It is a question for measurement to decide.

(*b*) What relation to distance has apparent motion on the face of the sky? Are the stars which appear to move athwart the sky nine tenths of a second per year three times nearer us than the stars which move three tenths of a second per year? It seems very probable that this is approximately the case, and that apparent motion is a better criterion of distance than apparent brightness. Whether this conjecture is correct or not can be settled only by actual measurements of distances.

The decision upon these points is a fundamental necessity in the stellar problem, because it opens the way for a much more powerful and economical solution of questions in relation to distribution in distance through the discussion of meridian observations. That way is, to some extent, open now, but we need that certainty upon

the correctness of the fundamental principle (*b*) or of some modification of it which can only be had through direct measurement of the distances of a large number of stars.

That such measurements in large numbers are practicable is demonstrated by the successful work of Gill and Elkin at the Cape and of Elkin and Chase at New Haven with heliometers, by Kapteyn at Leiden and Flint at Madison (Wisconsin) with the transit, as well as by other observers.

Some of the available methods for measurement of parallax are:

By the use of heliometers.
By the use of meridian transits.
By photographic methods.
By micrometrical methods.

All these methods have been tried extensively; the first has apparently proved most accurate; the last, formerly employed almost exclusively, would probably be discarded now by common consent. Photographic methods have not hitherto proved entirely satisfactory perhaps, and yet it seems to be the almost unanimous opinion of those in the best position to judge that this method offers the greatest promise for efficient work on a large scale when properly used. The method of exposing the same plate at three successive phases of parallax, suggested by Kapteyn some years ago, is the one which seems to offer the greatest promise of economy in labor and precision in the result. From existing evidence it does not appear that the photographic method is likely to be very effective upon stars brighter than the fifth magnitude.

The heliometer would probably be better suited for parallax measures of bright stars. This method, though extremely precise, is slow and costly. It has been employed at the Cape of Good Hope in measurements upon a few of the far southern stars; but, so far as is known, no work of the kind is now going on in the southern hemisphere. The Cape heliometer is now devoted to planetary observations upon a new plan, a work very appropriate to the original purpose of the Royal Observatory at the Cape of Good Hope. Therefore, with the exception of the aid which might hereafter be rendered by the Cape heliometer, the entire field for the determination of stellar parallax in the southern hemisphere is open to the Carnegie Institution, should it desire to enter it.

This work could be undertaken with advantage on almost any scale. Without any idea as to what means might possibly be avail-

able, we are unable to suggest a definite program other than the restricted one which follows.

In the first line it seems desirable that an attempt be made to measure the parallaxes of stars known to be in sensible motion. As the limit of such motion for a comparatively restricted work, one might take 0."2 or even 0."1. In order to identify the stars having such motions, and to measure the motions themselves, the sources of information are now very scanty. It would be almost a necessity to carry through (2) of the program—the meridian observation of telescopic stars down to the ninth magnitude. If this were not done it would still be necessary to make accurate meridian observations of all stars observed for parallax. The number which would be at present available under this plan and south of —20° of declination would probably be considerably under 500, though others could doubtless be found without much difficulty. We would recommend the use of a photographic telescope of relatively long focal distance for this purpose. It should be of the highest optical perfection. It is quite possible that a three lens or four lens combination would be advisable, in order to get sharp, round images over an area of at least four square degrees if possible. We suggest a telescope of 18 inches aperture and about 30 feet focal length.

It would also be highly desirable to measure with the heliometer the parallax of many stars brighter than the sixth magnitude and south of —20° of declination. A seven inch heliometer similar to those in use at the Cape and at New Haven would be suitable for this purpose. The stars selected should be chiefly those distinguished for proper motion—several hundred in number. This would be in effect an extension of the parallactic survey which has been carried on so successfully at New Haven within recent years.

We think that the services of two observers on this work for a period of at least eight years would result in an extremely valuable contribution to the solution of the stellar problem.

For some years the project of a general *Parallax Durchmusterung* has been brought to the attention of astronomers. This contemplates nothing less than the determination of the approximate relative parallax of every star down to the ninth or tenth magnitude. The inquiry would mean something like this: If we take as the unit of distance the average star of the ninth magnitude, what are the relative distances of other classes of stars, and what individual stars are especially near the solar system? A program for this purpose is described in the letter which Professor Kapteyn has addressed

to the Committee in response to its request (Appendix B). This would be an immense undertaking, which would seem to demand the cooperation of several agencies before it could be properly undertaken for the entire sky. It would also be desirable to learn more of the practical capabilities of the method proposed before making any recommendations upon the subject.

The plates which would be taken in the course of the attempt to determine the relative parallaxes of stars known to have sensible motion would themselves offer an opportunity for preliminary tests of the method and of the value of the expected result. It would also be easy to provide this in a more systematic form for restricted areas of the sky by the exposure and development of plates especially for that purpose, provided the measurements and computations for these plates could be arranged under a cooperative plan of limited extent, such as that suggested in the letter of Professor Kapteyn (Appendix B).

In view of these considerations we are of the opinion that it would be advisable, in connection with the proposed Southern Observatory, to set up a photographic telescope of about 18 inches aperture and 30 feet focal length for parallax work on the southern stars, and that it would probably be found desirable to maintain it in constant operation for a period of eight years at least. The services of two skilled observers and a small staff of measurers and computers would be required.

(4) *Measurement of Radial Motions.*

In close connection with the three projects already mentioned is another for the measurement of velocities of stars in the line of sight—*i. e.*, radial velocity toward or away from the earth. A knowledge of these velocities is of the utmost importance, and since the accurate measurement of such velocities has become possible, a most valuable source of information for verifying and enlarging the conclusions to be drawn from the discussion of proper motions has been placed in the hands of astronomers. In fact, through an adaptation of the investigations for solar motion in connection with these measurements of radial velocity, it is possible to obtain valuable conclusions as to the distances of various classes of stars having sensible proper motions. When a very large number of such measurements upon stars distributed over the entire sky shall have been obtained, it will be possible to determine the velocity of the solar motion with great precision. It will even be

possible to obtain in this manner an absolutely independent check upon the direction of solar motion which, in a problem so important, will possess the highest philosophical value and will become a valuable test of fundamental hypotheses as to structure and motion in the sidereal system.

Whether the stars are distributed with approximate uniformity in volume, and whether the motions are at random in every conceivable direction, are questions which require for a definite decision the added information which can be obtained by the measurement of radial velocities of large numbers of stars extending, if possible, somewhat below those visible to the unassisted eye, in combination with discussions founded on proper motions which result from meridian observations. What we now most need is such measurements for stars not visible from the observatories of the northern hemisphere. Scarcely anything in this line has been accomplished for the southern hemisphere. Recently the Lick Observatory has dispatched an expedition to Chile with the Mills three foot reflector to make such measurements, and the maintenance of this expedition has been provided for by Mr. D. O. Mills for three years. It may be expected to produce results of the highest importance—of importance relatively several times as great as would attach to like efforts with the same instrument in the northern hemisphere.

Thus we may hope to have at our disposal within a few years the measured radial velocities of practically all the stars in the whole heavens that are brighter than the fifth magnitude. This will be an extremely valuable result; but it would be made far more valuable if such measurements could be secured for a greatly increased number of stars over a greater range of magnitude.

Are the velocities of the more distant stars the same on an average as those of the nearer stars? Are the peculiar motions of the stars, after abstraction of parallactic motions, the same in the directions to and from the observer at different distances from the Milky Way? In order to answer these and similar questions with sufficient weight of evidence, the objects for which velocities in the line of sight have been measured should number 2,000, if possible; and in order to accelerate the rate at which results can be reached, we need telescopes of the largest possible light-grasp. There is no apparent obstacle, except cost, in the way of employing a telescope with a five foot mirror for this investigation.

That an increase in the resources for the measurement of radial velocities of stars in the far southern skies is desirable, appears from

the fact that, while there is permanently located in the southern hemisphere only one telescope which is used for this purpose, and provision made for the use of another during three years, there are employed in this service in the northern hemisphere at least six large telescopes, each more powerful than any telescope installed at a permanent observatory in the southern hemisphere. The disparity in resources here presented is marked, and the call for a remedy seems to be imperative.

It would therefore seem to be very desirable that the Carnegie Institution should enter this field and provide for use in the southern hemisphere the most powerful reflecting telescope that would be sanctioned by experience and the dictates of common prudence. The use of this in the measurement of radial velocities of southern stars should be provided for during six years at least. There would be needed a staff of two skilled observers and a half dozen measurers and computers.

The additional argument in favor of the provision of a large reflector to be used in the southern hemisphere will be found later, under (7), in this enumeration of proposed works.

The observations specified under the four preceding heads are closely related to each other, and logically they are branches of a single enterprise—an endeavor to make a strong forward movement in the solution of the sidereal problem. We consider it extremely desirable that provision for all the works enumerated by us should be made in the proposed Southern Observatory, but in the event of necessary curtailment which does not extend to the entire program, it is to be hoped that such curtailment may not apply to either of the four projects thus far mentioned.

(5) *Observations of Double Stars.—A Large Refractor.*

The measurement of double stars has been in active progress for more than a century. For the past seventy years work in this line has absorbed a very large proportion of the energies devoted to astronomical investigation. Certainly the class of facts developed by these investigations have remarkable interest, and they have exerted a deep influence upon the thoughts of man as to his place in nature. That the law of gravitation apparently extends throughout the universe; that suns revolve about suns; that the orbits are usually elliptical, like those of the periodic comets; that many of these bodies are larger and more splendid than our sun—these and

numerous other facts seem to lend a special significance to this branch of astronomy. Except incidentally, this work is not very closely associated with the stellar problem, and the immediate advantage of a greater extension of work in this line in the southern hemisphere is not so apparent; yet it should be remembered that no really large telescope has yet been applied for any great length of time to the measurement of double stars in the southern hemisphere, and while we put this project after the four already mentioned, we entertain no doubt of its desirable character.

It would seem very desirable that a telescope of about 27 inches aperture should be provided for this work. There should be a regular survey of the entire southern sky for the discovery of new double stars, to complete similar surveys carried out at the Lick Observatory and elsewhere.

Double-star work on the southern sky has come practically to a standstill, while it is still going on industriously at several observatories in the northern hemisphere. That a large telescope in the southern hemisphere should be devoted to double-star measurements for a period of about eight years seems to be evident.

One observer and one assistant would be required.

If a large refracting telescope should be provided for the proposed Southern Observatory it is very likely that certain micrometrical and other studies, apart from double-star observations, would be worth while, perhaps calling for the detail of another observer. This comparatively small expenditure would doubtless be well compensated in the increased utility of a costly telescope justified for another purpose.

Possible combination of (3) *and* (5).—In providing the instrumental means for carrying out (3) and (5) upon a restricted scale, it might be possible to combine the two telescopes required, so that one mounting and observing room would suffice. This would result in a very large saving in plant. The tubes of the two telescopes, each designed as perfectly for its peculiar purpose as if made for special instruments, could be attached to the same declination axis. Parallax plates should be exposed in the early evening and late morning hours, in any case. The result would be that the parallax telescope would usually be unused during the four or five hours around midnight, in order to avoid the use of the telescope at large hour angles east or west. During this period, if the parallax telescope were combined with the telescope for micrometrical observations the latter would be wholly free during four or five hours near midnight, the

hours which, on the whole, would be most suitable for the observer of double stars.

This arrangement is not suggested as ideal, but as one which might be adopted should the program for (5) be carried out and that for (3) be somewhat restricted. In that event we think the arrangement here suggested would be very much preferable, as it certainly would be very much more economical.

(6) *Variation of Latitude.*

The variation of the earth's axis of rotation is one of the newest and most interesting developments of astronomy. The facts relating to it have an intimate bearing upon the question of precision in meridian observations, in addition to the great interest which attaches to it as a physical phenomenon. Astronomy has not yet formed an adequate explanation of its origin. Quite recently Mr. Kimura, of the Japanese international latitude station, has called attention to a singular phenomenon developed by the international observations, which may be referred to several possible explanations, none of which appear quite satisfactory. In consequence of this, Dr. Chandler, editor of the Astronomical Journal, points out the necessity for establishing three observing stations in the southern hemisphere—one at the Cape of Good Hope, another at Sydney, and still another about 30 miles south of Santiago de Chile. This proposition has been heartily indorsed by several high authorities, including the Royal Astronomical Society. The operation of these three stations would tend to fix and define this anomaly in the observations beyond a doubt, and the result might be that the true cause of the phenomenon in question would be pointed out. Furthermore, the execution of this project would bring a most valuable contribution to the question of astronomical aberration.

We are strongly impressed with the importance of this work and are of opinion that, if the stations at the Cape of Good Hope and Sydney could be provided for by other agencies, the Carnegie Institution would do well to take the responsibility of the station in Chile. The annual expenditure would not be very great, and this project need not stand or fall with that for the Southern Observatory, although it has a logical connection with it.

The zenith telescope and observing shed required would not be costly, and two observers, without other assistants, would be able to take care of the observations and computations.

In addition to the undertakings we have already enumerated as offering a definite field of great usefulness for the proposed Southern Observatory, there are others which, in the judgment of many astronomers of high standing, deserve a prominent place in this enumeration. The following list briefly recapitulates some of these.

(7) *Astrophysical Researches.*

Should a large reflector be provided for the observatory in connection with (4) it could also be utilized with advantage for certain photographic and astrophysical researches of great importance to the advance of science. Among these are:

(*a*) Spectroscopic researches upon red and variable stars to supplement similar researches in the northern hemisphere.

(*b*) Photography of nebulæ in order to make the record of similar works complete for the entire sky.

(*c*) Spectroscopic examination of the nebulæ for the purpose of ascertaining their motions in the line of sight, as well as in the interest of inquiries into their physical nature.

These and similar works are named as supplementary to that mentioned under (4), not to be carried on to the detriment of the latter.

(8) *The Astrographic Chart.*

Allusion has already been made to the labors upon the "Carte du Ciel," or Astrographic Chart. Director Loewy, president of the Astrographic Congress, informs us that there is a vacancy in one of the zones; and he states that the services of the Carnegie Institution would be very acceptable in filling the vacant place. There is no question of the importance of this great project for securing a photographic representation of the entire sky at the present epoch. The work has already progressed so far that its ultimate success is now practically assured. It would be most unfortunate should the completion of one or two sections be delayed far beyond the completion of all the others, thus destroying the unity of epoch that is so desirable in all such works. This work requires the use of a thirteen inch photographic telescope of a special type. This instrument would not be very costly. The services of two observers and of a small corps of measurers and computers would be required during about eight years. We think that the feasibility of undertaking

this work ought to be taken into serious consideration by the Institution, should it be ascertained that there is no reasonable prospect of provision for it elsewhere.

(9) *Photometric Observations.*

Professor Seeliger urges the importance of precise photometry on the southern stars in the interest of problems relating to the structure of the sidereal system, and to supplement similar work carried on in the northern hemisphere. All the arguments which have been presented in relation to the first four numbers of this program apply to this. Statistical methods of investigation in this field have already led to significant and interesting conclusions, and we cannot doubt the power of this method, which has already proved such a valuable guide in affording reliable clues to the structure of the sidereal system. We think that Professor Seeliger's suggestion is worthy of further inquiry and consideration.

(10) *Researches of Minor Scope.*

A number of researches of minor scope have been suggested by our correspondents, all of which are of importance. For some if not all of them it seems desirable that interest should be excited at existing observatories in the southern hemisphere to take up these works and carry them to a successful conclusion. One of the good results which we should hope from the proposed Southern Observatory would be that it would serve to stimulate interest in astronomy throughout the populations of the southern hemisphere.

It will thus be seen that there is no lack of work of pressing importance to be done in the southern hemisphere. If an observing station should be established on a liberal scale for the execution of these works, it would still be a problem requiring wisdom and firmness to keep the program within practical limits and concentrated upon the furtherance of the great end desired.

The Question of Site.

We have given much attention to the question of a suitable site for the proposed Southern Observatory, but we are not yet prepared to make a definite recommendation.

For valuable information in regard to this question we are indebted to Mr. H. C. Russell, Government Astronomer at Sydney,

New South Wales; Mr. W. Ernest Cooke, Government Astronomer at Perth, West Australia; Dr. John M. Thome, Director of the observatory at Cordoba; Mr. Walter G. Davis, Director of the Argentine Meteorological Service, and Sir David Gill, Astronomer Royal at the Cape of Good Hope. We are greatly indebted to these gentlemen for the painstaking and valuable information with which they have favored us, for cordial offers of facilities, and for documents of interest which they have forwarded.

We have also devoted study to the meteorological and climatological reports in regard to the countries crossed by parallels of latitude suitable for the location of the observatory. In a preliminary way it appears to us that the most promising localities are in New South Wales, in the vicinity of Sydney; in South Africa near Bloemfontein, or on the Great Karoo plateau in Cape Colony, and near San Luis, in Argentina. San Luis appears to have a very clear sky and a salubrious climate. It is only 16 hours by rail from Buenos Aires. It is measurably free from the fearful "hondas" or stifling hot waves which characterize the Andean plateaus further west, and the skies are remarkably clear.

The latitude of Bloemfontein is rather smaller than is desirable, only 29° south, while —30° would seem to be almost the northerly limit admissible. Yet we have from Sir David Gill, who has inspected that locality in connection with the trigonometrical survey, the most favorable accounts of the wonderful transparency and steadiness of the atmosphere there, and of the remarkable number of clear nights, which is estimated at 300 annually, or about three times the number we experience upon the Atlantic seaboard. The elevation is about 4,000 feet above sea level. The mean annual temperature is, however, rather high.

Our early reports in regard to Australia were favorable, and we decided to procure a careful test of certain sites in the vicinity of Sydney. In April of the present year Professor W. J. Hussey, of the Lick Observatory, was appointed to make telescopic tests and other examinations with reference to observatory sites. He was engaged up to the end of July upon explorations in southern California, looking for a site for the Solar Observatory. Soon after the completion of this work he sailed for Sydney, on August 6, under instructions to test certain sites in the neighborhood of Sydney in relation to which we had formed favorable opinions, confirmed by the personal testimony of Mr. Russell. Mr. Hussey is provided with an excellent portable telescope of nine inch aperture, of which

the object glass was most obligingly loaned by The Alvan Clark and Sons Corporation of Cambridge, Mass., and of which the principal parts of the mounting were also loaned by the Lick Observatory, thus reducing the expense of the telescope to a nominal sum. With this telescope the character of the "seeing" is ascertained by systematic tests at all the stations visited. This can be compared with the excellent conditions prevailing on Mount Hamilton, where Mr. Hussey has had many years' experience.

Pending Mr. Hussey's examination and report, it scarcely seems worth while to enter into a detailed discussion in regard to sites at the present time. If the proposed Southern Observatory should be organized upon a scale sufficient to secure the execution of the greater part of the works enumerated in our program, the question of site would be very important indeed, and we have so regarded it. It is essential that the climate should be healthy for astronomers engaged upon tasks so strenuous; that there should be a large proportion of clear nights; that the air should be reasonably transparent and exceptionally steady, and that, so far as possible, the ordinary comforts of civilization should be found in the environment of the observatory. In the search for a stimulating and healthful climate, and also in the interest of the meridian observations, it would be desirable to choose a latitude of 40° south or more.

But in the southern hemisphere, in latitudes south of —35° or —40°, the amount of cloudiness is apt to be very great, or the climate is otherwise unsuitable. In other respects, as in what was formerly called Patagonia, some of the localities in high southern latitudes cannot be regarded as available. The vicinity of Hobart Town, Tasmania, offers many advantages, especially in the healthfulness and uniformity of its climate, but with the disadvantage of rain falling on nearly half the days of the year. The amount of clear weather in New South Wales, though superior to that of our Atlantic seaboard, is not quite all that could be wished. The object of the proposed observatory is to secure valuable observations in large masses by the expenditure of great energy during a comparatively short term of years. This qualification is not the only one, however, and may possibly be regarded as compensated somewhat if a locality can be found where the climate is healthful and where efficient routine assistants can be recruited from the surrounding population.

The funds appropriated for the use of the Commission will suffice for what it has undertaken to accomplish; but if site explorations

in South Africa or Argentina are desired, a further small appropriation will be necessary. Later, through the operations of Mr. Hussey and the kindness of Sir David Gill, the Committee expects to be in possession of better knowledge as to what may be advisable in this direction.

BUILDINGS.

The question of buildings and other constructions necessary for the proposed observatory is one which cannot be discussed in its minute details until something shall have been determined as to the site. Since the idea of this observatory is that of a temporary observing station, to be occupied, perhaps, not more than ten or twelve years, our ideas of the construction required would be largely controlled by that fact.

The necessity of providing for the equalization of the inner and outer temperature seems to prescribe for the observing rooms a form of construction which would not be very different whether the observatory were to be temporary or permanent. The essential principle is that the walls should consist of an iron or steel framework, with an outer covering of wood, in the form of louver work, and an inner covering of sheet metal, such as galvanized iron. The efficiency of this form of construction has been fully tested and seems to leave nothing further to be desired. For the drum to carry a large dome, this form of construction would probably be as economical as any other that could be accepted. Wood might be used for the framework were it not for the necessity of an even and solid construction for the tracks upon which the rolling mechanism of roofs and domes is supported. For the meridian instruments, sliding roofs should be provided. The great superiority of these over the old form of shutters is now fully demonstrated.

Since an important requirement for site is excellence of atmospheric conditions, it follows that the proposed observatory must be located at some distance from any large center of population. Consequently it would be practically unavoidable that provision should be made for housing the observing staff upon the observatory premises. This is an arrangement which is quite indispensable to the highest efficiency in any case. In any but an extremely exceptional climate there will be a large proportion of nights in which the probability of clear sky during the first half of the night will be doubtful. If observers live within easy access to the instruments, much clear sky will be utilized that will inevitably be lost otherwise.

Furthermore, after long duty at the instrument the observer is in need of rest, and in order to attain it should not be subjected to the hardship of a journey on foot for a mile or two in the small hours of the morning. For those observers who are obliged to begin duty at some time after midnight, it is practically indispensable that their residences should be in close proximity to the instruments they are to use. Aside from these obviously practical considerations, it has been found by experience that the plan of housing the staff upon the observatory premises effects a real economy in the quality and quantity of output in relation to the total expenditure.

We are of the opinion, therefore, that residences for the observing staff should be provided upon the site of the observatory. All that would be needed would be small cottages of simple construction, suitable to the climate. Equal simplicity ought to prevail in the construction of the office buildings required for administration, computing, library, and storage. Small work rooms would also be needed for the mechanical department.

Staff and Organization.

The question of organization for the proposed observatory is one which cannot be effectively discussed until something shall be known of the definite purposes of the Trustees in relation to its establishment. It goes without saying that no work whatever should be undertaken which cannot be put under the direction of assuredly competent and energetic astronomers interested in what they are to undertake. If the scheme should be inaugurated in its broadest scope, as we hope, it would be a mistake to begin the execution of the plans until the general control and direction can be arranged in a manner to command the confidence of the astronomical world, as well as that of the Trustees.

In some respects, however, the situation would be peculiar. In the ordinary case one of the most important functions of the directof of an observatory is the control which he exercises in the choice or work and methods. In the present case the director would be partly shorn of this privilege at the outset, since the very idea of the proposed observatory would be the performance of certain definite tasks.

Furthermore, in the selection of the staff for the execution of the full program it would be necessary to select, in addition to routine assistants, about twenty astronomers and assistant astronomers of proved capacity and experience in varying degrees. Other qualifications, such as health, energy, and capacity of adaptation to new

surroundings, would have to be considered in an unusual degree To recruit this staff all at once, for temporary service, from the existing forces of astronomy might prove to be a somewhat difficult task. In case this project should be adopted, therefore, the Institution should be prepared for a somewhat gradual organization of the observatory, extending possibly over four or five years, before all its departments should come into full action.

This delay, however, might prove unavoidable from another point of view. The necessary provision for instruments and for their proper installation on so large a scale is a matter which cannot suitably be disposed of all at once. In the light of previous experience it may be estimated that a period of three or four years, at least, would be necessary before the means for observation could be prepared in all its details.

The Southern Observatory as an Expedition.

An important part of the astronomical work of the southern hemisphere has been the result of special expeditions. There are the early expeditions of Halley and La Caille and the later ones of Sir John Herschel to the Cape of Good Hope; of Johnson to the island of St. Helena; of McClean to the Cape of Good Hope for astrophysical work, and of others. The expedition of Captain Gillis to Santiago de Chile in 1850–'53 resulted in several extensive series of astronomical observations. The establishment of the Argentine National Observatory originated in what was essentially an astronomical expedition of the most fruitful character under the conduct of Dr. B. A. Gould. The extremely valuable work of Stone at the Cape of Good Hope was also virtually that of an expedition for a particular purpose. Nearly all the other astronomers who have done highly valuable work in the southern hemisphere have been northern astronomers who went to southern stations for some special work.

The idea of a temporary observing station in the southern hemisphere will not, therefore, seem to be in any way strange. In one respect it embodies an extremely economical principle—the observatory would be maintained only for the accomplishment of works deemed highly important. There would be no chance for it to pass through stages of comparative inaction or to engage in work which is comparatively less pressing, or in undertakings equally well done elsewhere. These are the dangers that may threaten a permanent institution.

Furthermore, the duties of administration would be somewhat simplified. It would chiefly be necessary to provide the means for ascertaining whether the works adopted are pushed with the requisite energy and skill—whether the product is that which was stipulated.

Pushing this idea to an extreme, the work of the observatory would virtually consist of a series of expeditions having scarcely any connection one with another, except that of proximity at the scene of operations. As fast as the preparations for one of these lines of work should be complete, the expedition for that would be despatched upon its mission, with its own head and its own staff.

The larger the establishment, the more necessary it would be to provide a strong observatory organization.

All these questions can be more effectively studied after it becomes known upon what scale the enterprise can be carried out.

III. Observatory for Solar Research.

As the central body of the solar system, confining the planets in their orbits by the power of its attraction and supplying them with light and heat through its radiation, the sun possesses for us an interest greater than that of any other celestial body. From one point of view this interest may be considered to be of a most practical character, since the conditions of terrestrial life are determined exclusively by the solar radiation, so that any possible changes which this radiation may undergo are likely to be of consequence to life upon the earth. From another standpoint the study of the sun possesses a philosophical interest of the highest kind, for the sun is a star, comparable in all particulars with countless stars which lie beyond the boundaries of the solar system, but possessing the unique distinction, through its proximity to the earth, of being susceptible of detailed study and investigation. Thus in all reasoning on the physical constitution of the stars, especially in connection with the great problem of stellar evolution, we must start from the sun as a type object and elucidate stellar phenomena from an intimate acquaintance with solar phenomena. We have no foundation for the hope that any other star will ever appear larger than a microscopic point of light, even though the telescopes of the future may completely outrank the instruments of the present day; but through the provision of more adequate means of studying the sun

we may hope immeasurably to strengthen and deepen the foundations on which investigations of stellar phenomena are laid.

Conversely the only means of studying the origin and development of the sun and of determining what it will become in the future is afforded by the phenomena of stars and nebulæ, for we find in the heavens stars in all stages of growth, illustrating every step in the process of evolution by which the sun has been developed from a nebula. Solar research should thus begin with the nebulæ, proceed with a physical investigation of those celestial objects which represent the earlier stages of stellar growth, culminate in a study of the solar structure and radiation, and conclude with an examination of the red stars, one of which the sun will some day become. The study of the sun, with the inseparably connected question of stellar evolution, thus presents a single great problem, important alike to the philosopher, the astronomer, the physicist, the chemist, the geologist, and indeed to every one interested in the study of nature.

Purpose of a Solar Observatory.

After full and careful consideration of the recommendations of the Advisory Committee on Astronomy* and an extended examination of the various questions involved, we respectfully recommend the establishment by the Carnegie Institution of a Solar Observatory, so situated and equipped as to permit the accomplishment of three principal objects:

(1) To measure the intensity of the solar heat radiation, and to determine whether it varies from perfect constancy during at least one sun spot period of eleven years. In connection with this investigation, to measure the absorption of sunlight in its transmission through the atmosphere of the earth and that of the sun, and also the radiation of different portions of the sun's image, such as spots, faculæ, and prominences.

(2) To bring to bear upon the solution of solar problems various modern methods of research, principally of a spectroscopic nature, which have not hitherto been applied with adequate facilities. More specifically, to provide for the investigation of various solar phenomena with the spectroheliograph, the visual and photographic study, with powerful spectroscopes, of the spectra of the chromosphere, sun spots, and for other researches of a similar nature.

*Carnegie Institution Year Book No. 1, Appendix A, p. 96.

(3) To provide, through the construction of a large reflecting telescope, for the investigation of various problems of stellar evolution, intimately related to solar work, which existing instruments are inadequate to solve.

It will be seen that the investigations here proposed may be grouped in another way, viz., (1) those which relate to the sun's radiation, mainly with reference to its effect upon the earth; (2) those which relate to the solar constitution, with special reference to the sun as a typical star; and (3) those which relate to the evolution of stars like the sun from nebulæ.

There are many important reasons to recommend the establishment of a solar observatory by the Carnegie Institution. Up to about the year 1875 a large amount of information regarding the phenomena of the sun's surface had been collected, partly through the utilization of more and more powerful telescopes, and particularly through the recent application of the spectroscope. But since that time, for reasons not easily to be explained, comparatively few important advances in the study of these phenomena have been made; very little advantage has been taken of the great improvement in telescopes and in spectroscopes during the intervening quarter of a century. No other department of astrophysical research has been equally neglected, and consequently in none is there such an exceptional opportunity for great advances. Only one of the twenty two refracting telescopes of from 20 inches to 40 inches aperture is regularly used for work on the sun, and with but two or three exceptions the solar spectroscopes in use are little better than those of a quarter of a century ago. Though such spectroscopes are fairly well adapted for the statistical work in which they are employed, they are wholly incapable of dealing with phenomena easily within reach of such spectroscopes as are used in physical laboratories. Even in the few cases in which important advances in solar investigations have been made, partly through the invention and perfection of new instruments, the means available have generally been inadequate to bring out the full powers of new methods of research, and atmospheric disturbances have always most seriously hampered observation. What is needed is an observatory at some suitable mountain site, where atmospheric disturbances are reduced to a minimum; the development of special forms of telescopes, particularly adapted to solar work, and the complete utilization of the numerous improvements in spectroscopes and other instruments for physical research which have been developed in the

physical laboratory and require laboratory conditions for their successful use. No existing organization proposes to do this work under these conditions, and there is no prospect that it will be undertaken unless by the Carnegie Institution.

ADVANTAGES TO BE GAINED THROUGH IMPROVED ATMOSPHERIC CONDITIONS.

Up to the present time practically all observations of the sun have been made from the lower regions of the atmosphere. This surrounds the observer in a vast fluctuating mass, which reduces the brightness of the heavenly bodies by nearly one half, and transforms their images, which should be sharp and clearly defined, into boiling and confused objects, in which the delicate details of the originals are almost wholly concealed. At rare moments of comparative calm, glimpses may be had of structure of indescribable delicacy, but if partially revealed for a moment it is instantly swallowed up by disturbances in the atmosphere. It is as though the astronomer were forced to make his observations from the bottom of an ocean, whose constant storms are not confined to its surface, but penetrate the utmost depths, churning them into a seething mass, through which all external objects seem vague and ill defined. It is evident that such disturbances in our atmosphere must prevent not only a clear and perfect understanding of the solar structure, but in no less degree an accurate and reliable measure of the intensity of the solar radiation, which will seem to vary with the fluctuations in the atmospheric absorption.

A sharp distinction must here be drawn between two very different kinds of disturbances which the atmosphere produces.

(1) In the measurement of the sun's heat radiation, to determine whether it varies from year to year, the *absorption* of the atmosphere is the principal obstacle. No solar image is required, and local disturbances due to irregular refraction are of little consequence. The absorption may evidently be obviated in large part by making observations from the summit of a very high mountain, at a point well above the denser portion of the atmosphere where most of the absorption occurs.

(2) The detailed study of the various phenomena of the sun's surface, on the other hand, is impossible without a large and well-defined image, free from disturbances caused by local inequalities of temperature in our atmosphere. Currents of warm and cold air, especially if they are in the neighborhood of the instruments, are fatal to successful work.

The absorption of the atmosphere for heat and light radiations depends mainly upon the length of the air path which must be traversed by the rays. At an altitude of 15,000 feet the most fluctuating part of the atmosphere has been left below, and the rare atmosphere above is subject to comparatively little variation in its absorptive power. If, then, an observatory for the study of solar radiation should be established at some such height above the earth's surface, in a region where but little water vapor is present, the difficulties hitherto experienced in the measurement of the solar heat would in large measure disappear.

But it by no means follows that such a site would be suitable for that department of solar research which requires a perfectly defined image of the sun. As a matter of fact, the sharpness of definition experienced during the day on mountain tops is frequently much inferior to that which may be found at lower levels. At the summit of Pikes Peak (14,147 feet), for example, although the transparency of the atmosphere is very marked, the sun's image is usually not well defined. The same is true at Mount Ætna (9,650 feet), except in the early morning when the low sun has not yet greatly heated the mountain slopes. At the Lick Observatory, on Mount Hamilton (4,208 feet), the day conditions are better, and are probably similar to those which are found at the Yerkes Observatory (1,100 feet).

Until recently no mountain peak has been known on which the sun could be observed to advantage throughout the day. This is presumably due in large part to the fact that the mountain slopes, if not thoroughly covered with foliage, become greatly heated by the sun's rays, producing ascending columns of warm air, which rise toward the summit and mingle with the cooler currents brought by the wind. Under such circumstances bad definition would be inevitable. At Mount Lowe (5,650 feet), near Pasadena, California, in a region where the remarkable uniformity of temperature and pressure would lead one to expect good definition, the solar image is frequently disturbed by currents of warm air rising from the unprotected slopes. Separated from Mount Lowe only by the width of a single canyon is Mount Wilson (5,886 feet). This mountain is well covered to the very summit with foliage, and thus stands in marked contrast with many of the mountains in southern California.* In consequence of this fact, all of the advantages to be expected from the exceptional quality of the atmosphere are experienced, without the disadvantages due to warm air rising from the heated

*See Professor Hussey's report, Appennix A.

slopes of the mountain. It is not surprising, therefore, that the sun's image, as seen from the summit of Mount Wilson, is apparently better defined than at any other point hitherto tested with a telescope.

But for the successful prosecution of solar research another condition must be fulfilled. The phenomena on the sun's surface are constantly changing in form, not only from hour to hour, but from second to second in the violent eruptions which are numerous during the period of greatest solar activity. In order to study these changing phenomena intelligently, it is necessary that they be kept constantly under observation. In observatories subject to frequent clouds and storms the progress of such solar changes cannot be steadily watched. At some critical moment clouds frequently interpose to prevent further work. In an investigation of the solar rotation, for example, it is of great importance that the position of a sun spot or a facula be determined day after day without interruption. In actual practice many of the photographs made at existing observatories are rendered almost useless on account of the cloudy periods which separate them from other photographs. Mount Wilson has the unique advantage of combining extraordinary perfection of definition with such freedom from clouds as to permit continuous work for months at a time.

Summing up, we may therefore say that, even with existing methods of research, important advances in our knowledge of the sun could be attained by providing for observations (1) of the solar heat radiation from some high elevation, and (2) of the phenomena of the sun's surface from a site such as Mount Wilson. In the beginning the work should be divided between two sites, but it is probable that the higher station could be given up after the relation of the atmospheric absorption at the two stations should become known. During the same period it would also be necessary to provide for simultaneous observations from a third point many thousands of feet below the high station, to measure the absorptive effect of a known atmospheric layer, in order that the total atmospheric absorption may be determined and eliminated.

New Types of Reflecting Telescopes and Their Use in Conjunction with Laboratory Instruments.

The exceptional opportunity which exists at the present time for advancing our knowledge of the sun by no means depends solely, however, upon the possibility of eliminating a large part of the disturbances due to our atmosphere. Even greater possibilities for

advancement lie in the application of new instruments and methods. Of first importance is the development of the telescope, including: (1) its construction in the horizontal form, especially for work requiring a large solar image and the use of spectroscopes and other instruments from the physical laboratory; and (2) its construction as a large, short focus reflector, equatorially mounted in the *coudé* form, and particularly adapted for the photography of nebulæ, the investigation of stellar spectra, and the study of the heat radiation of the stars.

Astronomical telescopes are of two kinds—refractors and reflectors. The former consist essentially of a lens mounted at the upper end of a tube, which is pointed toward the object to be observed. The lens forms, at the lower end of the tube, an image of the object the size of which varies directly with the length of the tube. Reflecting telescopes, on the other hand, consist of a concave mirror, usually of silvered glass, supported at the lower end of a tube which is open at its upper end. The rays from the object fall upon the mirror, which reflects them back and forms an image at the upper end of the tube. By means of an additional mirror this image is reflected out at one side of the tube, where it may be observed. In both types of telescopes the tubes are pointed directly at the object under observation, and the apparent motion of the object through the heavens is counteracted by a uniform motion of the telescope, produced by clock work.

The development of reflecting telescopes during the first half of the nineteenth century culminated in the great instrument of Lord Rosse, erected in 1845. The crudeness of the mounting of this telescope, due to the lack of suitable engineering facilities, rendered it useless, except for such visual observations as could be made in the absence of a driving clock. Partly for this reason the immense advantages of mirrors over lenses were not discovered, and during the latter part of the century attention was concentrated in large measure on the development of refracting telescopes. These advanced rapidly in size, from the 10-inch telescopes of Fraunhofer at the beginning of the century to the 15-inch Harvard telescope (1847), the 36-inch Lick telescope (1888), and, finally (1897), the 40-inch telescope of the Yerkes Observatory.

(1) But as equatorial telescopes increase in size, it becomes more and more evident that a limit must be set to development in this direction. The driving clock of the Yerkes telescope must move a mass weighing twenty tons with such precision that the image of

the sun will remain fixed in the field of view for hours together. Attachments weighing as much as 700 pounds may be carried at the lower end of the tube, but it is out of the question, in spite of the great size and strength of this telescope, to carry the large spectroscopes of modern times. Through the work of Rowland, whose construction and use of concave and plane gratings has done more than any other one thing to revolutionize spectroscopy, the spectroscope of the physical laboratory has become an instrument of large proportions and of correspondingly great power. Such an instrument may be as much as 40 feet in length, and even if it could be attached to a telescope, the bending of the spectroscope, resulting from its constant changes of position, would render it impossible to obtain sharply defined photographs of spectra. The changes of temperature which occur from hour to hour in an open dome, subject to the fluctuations of the outer air, would also interfere with the use of such spectroscopes for photographic work, even if their rigidity were perfect. As a consequence, the solar spectroscopes in use today are in almost every case practically identical with the instruments of a quarter of a century ago. Moreover, many physical instruments of recent invention and of extraordinary power are so constituted that they cannot be attached to a moving instrument: they must stand absolutely at rest, protected from the most minute disturbances, on massive piers, in a constant temperature laboratory. It is evident, therefore, that a telescope for physical work, if it is to be suitable for use with such instruments, must be so constructed as to bring an image of the sun or of a star into a physical laboratory provided with all appliances necessary to protect the delicate instruments from vibrations, from relative flexure of their parts, or, sometimes, from temperature changes of even a few hundredths of a degree, and to meet other requirements demanded in the most refined research.

The lack of a suitable horizontal telescope made it necessary for Rowland to confine his spectroscopic observations to the light of the sun as a whole, though the use of his powerful spectroscope for a study of the solar details would have yielded results of the greatest importance.

Other requirements of solar work remain to be mentioned. It has been found that a solar image seven inches in diameter offers decided advantages over a two inch image. There can be little doubt that with such atmospheric conditions as exist on Mount Wilson solar images of two feet or possibly even three feet in diameter

could be advantageously employed at times. The use of such large solar images should immediately permit advances of importance to be made. But it would be wholly impossible to secure such images with a refracting telescope of the ordinary type, for the corresponding lengths of the telescope tube would be 215 feet or 322.5 feet, requiring mountings of enormous dimensions, not only excessively costly, but wholly beyond the possibilities of construction. For this reason it is evident that another type of telescope must be employed if such large focal images are to be used.

Fortunately it is possible to meet all the demands imposed by the above named conditions. It is only necessary to mount the telescope tube horizontally in a north and south direction and reflect the light of the sun into the tube by means of a plane mirror driven by clock-work. This mirror is the only moving part of the entire mechanism. Its small size, as compared with a moving tube hundreds of feet in length, not only renders the problem of following the object a simple one, but it eliminates at once the entire question of the enormous cost of the moving tube, the dome, and the great elevating floor which, with an equatorial telescope, would be necessary in order to permit the observer to reach the lower end of the tube in all its various positions.

Heliostats of various kinds have been used for many years, but until recently no serious attempt has been made to construct a large horizontal telescope. The instrument of this type built for the Paris exposition was never completed, and its design was such that the conditions demanded in solar work could not have been met. The recently completed Snow horizontal telescope of the Yerkes Observatory and the horizontal telescope of the Smithsonian Astrophysical Observatory represent the type of telescopes here recommended for solar research. The Smithsonian instrument was designed for bolometric work, and it has proved to be admirably adapted for this purpose. The Snow telescope was designed for solar investigations requiring a very sharply defined solar image, and special precautions were taken to secure this result. Such a telescope accomplishes, at very small expense, the purposes already named; it brings a fixed image of the sun into a laboratory, where it may be observed with large spectroscopes or other apparatus mounted on piers.

These instruments demonstrate the possibility of constructing a much larger telescope of the same type, designed especially for solar work and provided with large spectroscopes and spectroheliographs. To be successful such an instrument should be mounted at a consid-

erable height above the ground, at a site like Mount Wilson, where the atmospheric conditions during the day are exceptionally fine. Under such circumstances a sharp and well defined solar image, from two to three feet in diameter, could be obtained, and numerous researches now entirely out of reach could be undertaken with every reason to hope for success.

Dr. Elihu Thomson has suggested that any distortion of the mirrors by the sun's heat can be obviated by making them of fused quartz, since the coefficient of expansion of this substance is almost inappreciable. Since Dr. Thomson has already succeeded in making small mirrors in this way, we include an item to cover the expense of the necessary experiments, which Dr. Thomson has very kindly volunteered to superintend.

(2) In work on the sun, as already remarked, the most important requirement is a large solar image produced by a telescope of great focal length, but in order to trace out the successive stages in the development of stars like the sun, a telescope of a very different type is required. The remarkable opportunities for advance in astronomy which exist at the present time through the possibility of building a large reflecting telescope were outlined in the Year Book of the Carnegie Institution for 1902 (p. 141). The optical parts of the Snow horizontal telescope consist exclusively of mirrors, and it thus preserves the peculiar advantages of the reflecting telescope; but for the photography of faint nebulæ, and for many other similar researches of fundamental importance in the study of the sun's origin and development, this type of telescope is not well adapted. What is needed is a mirror of the largest possible diameter and of short focal length, provided with a heavy and well constructed equatorial mounting.

To give an idea of the immense advantages of an instrument of this kind it may be recalled that with a two foot reflector an exposure of 40 minutes suffices to photograph stars that are invisible in the largest refracting telescopes. With longer exposures millions of stars can be photographed with such a reflector, of whose existence the largest refractors could never give any indication. A 5-foot reflecting telescope would collect six times as much light as a 2-foot, and nearly three times as much as the largest reflector now in use, and would open up certain fields of investigation now entirely closed. It would furnish means of photographing the nebulæ which would probably be superior to those offered by any existing telescope. It would also permit the heat radiation of some of the brighter stars

to be measured, from which valuable conclusions could be drawn as to their physical nature, and it would furnish the essential means of studying stellar spectra on a larger scale than that afforded by existing instruments.

Up to the present time such powerful grating spectroscopes as those of Rowland have not been employed for the study of stellar spectra. As already stated, the spectroscopes are too large to be adapted to equatorial telescopes, and the feeble light of the stars would demand exposures far longer than can be given under present circumstances; but with a great reflecting telescope, so mounted as to produce the image of a star in a constant temperature laboratory, there should be no serious difficulty in photographing the spectra of the brightest stars with the most powerful grating spectroscopes. The exposures might have to be prolonged for several nights in succession, but it would only be necessary during this time to maintain the spectroscope rigidly mounted on fixed piers at a constant temperature. With such photographs of stellar spectra a large number of problems, of great importance in connection with questions of solar physics, could be solved. For example, it would be possible to determine beyond question whether, as is now believed by some investigators, the red stars have on their surface a great number of spots like those of the sun. If the presence of numerous sun spots on these stars could be proved it would follow that as the sun grows colder, and advances toward the condition of the red stars, the spots on its surface will multiply in number. Such a conclusion would have a very important bearing on the problem of the solar constitution. Many other similar questions could be answered, such as those which relate to the relative pressure in solar and stellar atmospheres, the changes in the solar spectrum which will result from decreasing temperature, etc.

General Nature of the Principal Problems of Solar Research.

It should be evident from what has been said that improved instruments and methods of research, employed under atmospheric conditions more favorable than those experienced at existing observatories, should render important advances possible. Let us now consider briefly some of the principal problems of a solar observatory.

The Constitution of the Sun.

The problem of the solar constitution, though repeatedly attacked on both observational and theoretical grounds, still remains unsolved.

In addition to the bolometer, referred to elsewhere, the two principal instruments employed for this work in conjunction with the telescope are the spectroscope and the spectroheliograph. The former permits the investigation of the nature of the chemical elements in the sun, their physical condition, and their motions in the direction of the earth, while photographs taken with the latter show the distribution of the various elements in the solar atmosphere and on the sun's disk. The two instruments supplement each other most effectively, and can be used to great advantage in connection with the bolometer and other physical apparatus.

The sun closely resembles the earth in chemical composition, but differs from it in every other particular. Intense heat, indefinitely greater than that of a Bessemer converter, maintains its substance in a state of vapor. The visible surface of the sun marks the limit where the metallic vapors of the interior, coming into contact with the cold of space, condense into luminous clouds. This surface presents a granular appearance, since the bright clouds form the upper extremities of columns of vapor ascending from the interior, separated by spaces filled with cooler and less luminous vapors. Above the visible clouds columns of uncondensed vapors continue to ascend. Hydrogen, helium, and calcium rise to heights of several thousand miles, and at certain points project from the nearly continuous sea of flame (the chromosphere) in the form of great gaseous prominences, ranging in altitude from 15,000 to 300,000 miles. At times of greatest solar activity violent eruptions frequently occur, producing prominences which sometimes rise to a height of nearly 300,000 miles in less than half an hour.

Formerly the flames of the chromosphere and prominences were visible only at the sun's circumference, when at times of total eclipse the dark body of the moon intervened to cut off the overpowering illumination of the earth's atmosphere. In 1868 it was found that they could be observed in full sunlight with the spectroscope, and in 1892 they were first successfully photographed with the spectroheliograph. This instrument also permits the flames to be photographed in projection against the sun's disk, thus rendering possible the investigation of a great variety of new and remarkable phenomena. The invisible vapors of calcium, and recently, through the application of the Rumford spectroheliograph of the Yerkes Observatory, those of hydrogen, iron, magnesium, or any other substance present in the chromosphere, can be photographed at will, and their forms, distribution, and motions investigated.

The peculiar differences in the behavior of the various elements and the characteristic parts they play in solar storms suggest questions which are quite as important to the chemist or the physicist as to the astronomer. The high temperature and the enormous masses of material involved in these solar phenomena far surpass the possibilities of laboratory experiments. If the elements can be broken up into simpler forms by intense heat, as the tendency of modern research seems to indicate, the best chance of detecting evidence of such dissociation would appear to be in the sun. If calcium, for example, can be separated by heat into several constituents, these can be photographed with the spectroheliograph in solar storms, where their difference in behavior should betray their separate existence. Results already obtained at the Yerkes Observatory point to interesting possibilities in this direction; but, in order to deal with the problem successfully, apparatus much more powerful than that now employed must be available for use under superior atmospheric conditions.

Problems without number relating to the solar constitution are ready for solution and demand only a carefully planned attack under suitable conditions. The peculiarities of the solar rotation, rapid at the equator and decreasing toward the poles, have been but little studied. The distribution of the elements in the lower chromosphere and the explanation of the absorption which produces the dark lines of the solar spectrum have been subjects of dispute for many years. This problem is now studied only at eclipses, but a large solar image, observed on any clear day with a powerful spectroscope in a good atmosphere, would permit important advances to be made. The true cause of the darkness of sun spots is not yet understood, and it is even uncertain whether they are cavities or elevated regions. Their minute structure and the remarkable phenomena of their chemical composition will afford indefinite opportunity for research.

All of these questions and many others are closely dependent upon the sun spot period. As the spots increase in number and activity all other solar phenomena vary in sympathy with them, increasing toward the period of greatest intensity and then fading away toward the time of calm. The investigation should therefore extend over a term of at least eleven years, and preferably until after the maximum which occurs about 1916. The advisability of continuing the work longer may be left to be determined in the light of the results obtained.

The Heat Radiation of the Sun.

The advantages of studying the heat radiation of the sun at high altitudes are well illustrated by the results obtained by Langley from the summit of Mount Whitney in 1881. The dryness and purity of the air at this elevation in the Sierra Nevada are perhaps unsurpassed at any other mountain station. Over one half of the atmosphere lies below the summit of the peak, and this comprises the denser and more variable strata, which interfere most with determinations of solar radiation. No sooner had Langley's delicate apparatus been set at work than a new class of solar rays previously unknown was discovered. Previous estimates of the absorptive effect of the lower regions of the atmosphere were found to be far below the truth, and the value of the solar constant, which measures the amount of heat received by the earth from the sun, was immediately increased by about one half. The new radiations which were found to possess so great importance cannot pass through glass, and prisms of rock salt must be employed in studying them.

The Mount Whitney observations extended over only a few weeks, and were made with apparatus which, from his present point of view, Professor Langley would consider extremely imperfect. In spite of the unfavorable conditions that prevail at Washington, Langley has continued his investigations at the Smithsonian Institution and developed his methods to an extraordinary degree of perfection. Slow and inaccurate observations with insensitive instruments have given place to automatic records of the highest precision, secured by the aid of photography with instruments so sensitive that differences of temperature of less than one ten-millionth of a degree centigrade can be detected.

The problems which should be attacked from the summit of Mount Whitney, or some equally good station, with the refined instruments now available, cover a wide range. The most important single question concerns the intensity of the solar radiation. Is this constant, or does it vary during that well defined period of about eleven years in which solar phenomena are known to pass from a state of comparative calm to one of violent activity, and then again to subside to their former condition? At times of sun spot minimum the sun's surface for months together is wholly devoid of spots. Faculæ and prominences are few and inconspicuous, and the spectroscope shows little, if any, evidence of disturbances of any kind. Gradually, however, spots begin to appear, and then rapidly multiply in number.

The sea of flame which surrounds the sun increases in brilliancy and activity. As the spots continue to increase in number and area, eruptive phenomena on a tremendous scale become more and more frequent. At the time of maximum activity the violence of these disturbances and the rapidity with which flames hundreds of thousands of miles in height form and disappear surpass all comprehension. In view of these facts it is not surprising that the question has been raised whether the total radiation of the sun does not undergo variations corresponding in some measure with these variations in the violence of the phenomena visible on its surface.

The solution of this problem, as has already been pointed out, requires that observations be made under conditions not now available. With instruments of the modern type installed at a great altitude and with similar instruments at a second station some thousands of feet below, the principal conditions needed for the solution of this question would be provided; for it is necessary not only to measure the intensity of the sun's heat from an elevation so great as to eliminate most of the obstacles interposed by the denser and more fluctuating portions of the earth's atmosphere, but also to arrive at a more thorough understanding of the absorption which solar rays undergo in passing through our atmosphere, and particularly to determine the difference in the quality and degree of this absorption at different levels. The study of the earth's atmosphere is of great importance in connection with this research. Indeed, it is not improbable that after the completion of a thorough investigation, carried on simultaneously at the upper and lower stations and extending over a sufficient period of time, it would be possible to accomplish all of the purposes of a solar observatory at a lower station.

When observed in the telescope the sun's disk is found to be much more brilliant at the center than near the circumference The difference is so great that it was detected in the earliest observations of the sun, made with the imperfect instruments of the time of Galileo. This difference is due to an absorbing atmosphere which completely surrounds the sun, and reduces the intensity of the light and heat radiated outward through it. In a study of this absorption made by Vogel in 1877 it was found that at the edge of the sun's disk only about 13 per cent of the violet rays escape. The percentage of transmitted light increases progressively for the blue, green, and yellow rays, until it amounts to 30 per cent for the red. From these results it has been concluded that if the absorbing atmosphere were

removed the intensity of the sun's light would increase by as much as 1½ times for the red rays to 2½ times for the violet rays. Under such circumstances the color of the sun would appear blue. Later results indicate that the removal of the sun's atmosphere would increase its radiation of heat about 1.7 times. It is evident that if there were any considerable variation in the absorptive power of the sun's atmosphere, due to such changes as might easily take place during the passage from minimum to maximum solar activity, the total radiation of solar heat might be very appreciably increased or diminished. Indeed, such a variation in the solar absorption is considered by Halm, in his recent memoir on the solar constitution, to be the cause of the eleven year sun spot period. It is therefore evident that the plan of observations should include an investigation of the heat radiation of various parts of the sun's disk, carried on during a term of years.

Evolution of the Sun and Stars.

We have already called attention to the fact that while telescopic observations may give us an intimate acquaintance with the sun's surface phenomena, from which conclusions may be drawn as to its physical constitution, they can give us no direct information as to the past or the future of the sun. We must seek such information in the stars and nebulæ. Keeler's work with the Crossley reflector of the Lick Observatory brought out the extraordinary fact that there are in the heavens at least 100,000 nebulæ, the majority of which are of spiral form. It is the belief of many astronomers that this spiral form is indicative of a process of development, and that the teachings of the nebular hypothesis must be modified so as to accord with this new point of view. Advantage should be taken of every possible increase in the scale of the photographs by which the forms of spiral nebulæ are recorded, in order that further data may be obtained that may be of service in future attempts to modify the nebular hypothesis. Photographs taken with all possible instrumental refinements might well be expected to show changes in the nebulæ after the lapse of a comparatively short period of time.

While we may thus seek in the nebulæ for evidences of the sun's origin, its early life and subsequent development may be traced in the stars; for, in spite of their enormous distance from the earth and the consequent impossibility of observing their surface phenomena as we do those of the sun, the spectroscope is competent to

give reliable information as to their physical condition and state of development. The secular changes of any star are so slow that thousands of years might be required to detect them. But it fortunately happens that all stages of growth are now represented. It is as though one were to pass through an oak forest and seek to learn of the development of the trees. During the period of his stay no apparent change would occur. But he would have before him trees of the same species in every stage of growth: the acorn, the sapling, the oak in its prime, and the dying tree, representing the last stage in the evolutional process. By observing these, the evolution of a single tree could be understood. Thus in the stars, while their changes are too slow to be observed, a similar opportunity exists of tracing their life history. Preceding the condition of the sun we find stars like many of those in the constellation Orion, which have advanced but little beyond the state of nebulæ. Next to these come white stars like Sirius, which are but slightly condensed, and represent what the sun must have been millions of years ago. From these it is possible to pass by gradual steps through the process of development which leads to the production of yellow stars like the sun. Here the process of decline has apparently set in, and the beginnings of the last stage are faintly visible. Finally come the orange and red stars, the spectra of which reveal phenomena of the greatest interest, such as the sun will exhibit after the lapse of many centuries.

Existing instruments have sufficed to develop the main lines in this process of evolution, but there are numberless questions which cannot be answered without the aid of much more powerful telescopes. A great reflector, such as would be most suitable for photographing the nebulæ, would also be much superior to any existing telescope for the spectroscopic investigations which furnish the data required in this research. Reference has already been made to the possible existence of great sun spots on the red stars; these and many similar questions of equal importance in connection with the constitution of the sun cannot be solved until such instrumental means have been provided.

Throughout all of this work, both on the sun and the stars, it would be necessary to have constant recourse to laboratory experiments in order to interpret the observed phenomena. A suitable equipment of physical apparatus for this purpose is therefore essential in connection with a solar observatory.

Plans and Estimate of Cost.

As already explained, a principal station and two temporary auxiliary stations will be needed for the proposed Solar Observatory. We recommend that the principal station (A) be established at some such point as Mount Wilson, where Professor Hussey's observations have shown that the conditions are excellent for work on the sun requiring very perfect definition of the image. For the study of the solar constant and the absorption of the earth's atmosphere two auxiliary stations (B and C) will be required for use during the summer months. Station B should occupy a site at the summit of a high mountain, while station C should be near the base of the mountain, some thousands of feet below. In view of the time and expense required, Professor Hussey's study of mountain sites did not include the very high mountains. We have obtained much valuable information regarding conditions on Mount Whitney and other mountains, but a further study of the matter would be required before the sites for stations B and C could be decided upon. While Mount Wilson would probably serve the purposes of the principal station, there should nevertheless be a further and more prolonged study of sites for this purpose, to be made while the equipment is under construction. Recent experience indicates a probability that stations B and C might be discontinued after observations have been made at these points for two or three seasons, since it is probable that all the work of the observatory could then be done at such a point as Mount Wilson.

Principal Station (A).

The summit of Mount Wilson is about 8 miles in an air line from Pasadena. At present it is reached from the base of the mountain by either one of two trails, suitable for pack animals, but not adapted for wagons. One of the first considerations, therefore, would be the problem of providing adequate transportation facilities. This might be done (1) by constructing a wagon road from the foot of the mountain to the top; (2) by constructing an electric railway from Pasadena to the summit of the mountain; (3) by extending to Mount Wilson the electric railway that now terminates on Mount Lowe. Owners of the property on the summit of Mount Wilson state very positively their conviction that one of the two latter projects would be carried out in case this site were selected. This would naturally be an important consideration in the final decision

upon the question of site. Estimates of the cost of construction are based on the assumption that such means of access will be available.

Plan of Work.

The general plan of work which we believe should be undertaken at this observatory would comprise the following classes of observations. This program is of course subject to such modifications as experience may suggest.

(1) Frequent measurement of the solar constant, together with studies on the absorption of the solar atmosphere and the radiation of different portions of the sun's image, such as spots, faculæ, and prominences. The principal instruments needed for this research are a 16-inch cœlostat and large spectro-bolometer for the solar constant work, and a 30-inch cœlostat, with concave mirror of about 200 feet focal length, for providing a solar image suitable for detailed radiation work.

(2) Systematic observations, with large spectroscopes and spectroheliographs, on such problems as the solar rotation, the structure and nature of sun spots, faculæ, etc., and other problems related to the solar constitution. For this work there will be required two 30-inch cœlostats, used in conjunction with objectives and mirrors ranging in focal length from 64 feet to 200 feet; two large plane grating spectroscopes, having focal lengths of about 21 feet and 42 feet respectively, provided with auxiliary apparatus for work with the spark and arc; a three-prism spectroheliograph of about 10 inches aperture, and a three-prism spectroheliograph of 8 inches aperture and about 33 feet focal length. In order to secure the best definition of the solar image, the cœlostats should be mounted at a considerable height above the ground, and electric fans should be provided for stirring the air by Langley's method.

(3) Astrophysical researches on stars and nebulæ with a large reflecting telescope, provided with a three-prism spectrograph, and also with a concave grating spectrograph mounted in a constant temperature laboratory, for use with the reflector arranged as an equatorial *coudé* in photographing stellar spectra with very high dispersion.

(4) Laboratory investigations, mainly of a spectroscopic nature, on problems arising in connection with the solar and stellar work. Much of the apparatus provided for this purpose should be mounted and used in connection with the solar spectroscopes.

Buildings.

The buildings for the observatory should be of the simplest construction, designed for results, rather than for appearances.

A. Spectroscopic and Bolometric Laboratory.—Experience has shown that in order to obtain good definition during the day the instruments should be mounted at the greatest practicable height above the ground. In the preliminary design for a separate spectroscopic laboratory, the cœlostat piers were accordingly carried up to a height of 40 feet. A sketch was also made for a separate bolometric laboratory. Subsequently, from motives of economy, a combination was made of these two laboratories, wherein provision is made for one 16-inch and two 30-inch cœlostats. The two large cœlostats would be used for work requiring an image of the sun—one of them for spectrobolometric observations, the other for work with the solar spectroscope and spectroheliograph. The 16-inch cœlostat would be employed for measurement of the solar constant. The spectroscopes and spectroheliographs are mounted on piers so high as to be above the level of the constant temperature house for bolometric work. The piers may be constructed either of steel, properly ballasted, or of granite, which is found in abundance near the summit of Mount Wilson. The laboratory itself is of wood, supported on steel construction, anchored to the rock. In the plans submitted provision is made for mirrors of 200 feet focal length, but it is probable that it would ultimately be desirable to extend the house 100 feet to the north, thus permitting the use of a mirror of 300 feet focal length. The construction of the building is such as to reduce to a minimum the heating of the walls and the consequent currents of warm air.

B. Reflector Dome.—For a reflecting telescope of 5-feet aperture, mounted in accordance with the general plan shown in the drawing,* a dome of 50 feet internal diameter is required. The walls of the tower are built of thin she et iron, the track for the dome being supported upon columns of steel construction. The sheet iron wall is covered on the outer surface with wooden louvers in order to prevent heating by the sun. South of the telescope pier is a constant temperature laboratory, with piers for the concave grating spectroscope.

C. Office Building.—This includes offices and computing rooms for all the members of the staff, together with shops for instrument construction, library, laboratories, photographic rooms, etc., suitably

* Not here reproduced.

equipped for work. This building might be constructed of wood or possibly of rough fragments of granite. In any event, provision should be made for storing the photographs and other records in a small fire proof room.

In view of the isolation of the observatory and the important purpose of developing new methods and apparatus in connection with its work, provision has been made in the estimates for a very complete equipment of the instrument shop. It is understood that this machinery would be purchased and immediately installed in a shop if the Carnegie Institution were to decide to establish the observatory. The tools would therefore be used from the outset in the construction of a large part of the equipment.

D. Dwellings for Members of Staff, etc.—Simple cottages and bachelors' quarters should be provided at the observatory site.

Other items of expense of a general nature would include a telephone line and a line for transmitting electric power to the summit of the mountain, sewer and water systems, etc.

STATIONS B AND C, FOR SOLAR OBSERVATIONS AT HIGH ALTITUDES AND STUDIES OF ATMOSPHERIC ABSORPTION.

No recommendation is made at present as to the site of these stations, although it is possible that Mount Whitney and Lone Pine would prove to be suitable. The estimates of expense are based on the assumption that sites similar to these would be selected.

The plan of work at these stations would include simultaneous observations for the measurement of the solar constant and the determination of the atmospheric absorption. The buildings of the two stations should be alike, except for the addition of a few rooms required at the lower station. They contain, in addition to shelter for the instruments, small dwelling rooms for the four men—two at each station—who would occupy them throughout the summer months. The instrumental equipment of the two stations would be similar, consisting of a 16-inch cœlostat and spectro-bolometer for the solar constant work, together with a small miscellaneous equipment of instruments required in connection with this investigation.

General items of expense in connection with stations B and C would include means of communication between the stations, simple devices to maintain the constancy of temperature needed for bolometric work, provision for water supply and fuel, improvement of trails, shelter for laborers and pack animals, etc.

Staff.

A director, in general charge of all work, with special duties in connection with solar spectroscopy.

At Principal Station (A).—

Bolometry: An observer in charge of all bolometric work, with special duties in connection with the spectro-bolometer; an assistant observer at the spectro-bolometer, and four computers.

Solar spectroscopy: Two assistant observers, to carry on, with the director, solar spectroscopic work; two computers at the beginning, with the probability that this number will have to be increased during periods of great solar activity.

Stellar spectroscopy: One associate and one assistant observer; one computer, and one night assistant, on duty with the large reflector.

In addition, there would be required a secretary-librarian, who would also have charge of the accounts and the purchasing of supplies; a stenographer, an instrument-maker, a skilled machinist, a carpenter, a janitor, and two laborers.

At Station B.—One observer, one assistant observer, and a man-of-all-work during the summer months.

At Station C.—A staff like that at Station B, with the addition of one instrument maker.

LEWIS BOSS, *Chairman.*
W. W. CAMPBELL.
GEORGE E. HALE.

OCTOBER 8, 1903.

APPENDIX A TO REPORT OF COMMITTEE ON OBSERVATORIES

REPORT BY W. J. HUSSEY ON CERTAIN POSSIBLE SITES FOR ASTRONOMICAL WORK IN CALIFORNIA AND ARIZONA

Prof. LEWIS BOSS,

Chairman Astronomical Committee, Carnegie Institution.

SIR: Acting under your instructions, I left San Jose for southern California on April 16, 1903, to examine into conditions for astronomical work in that section, especially with reference to research upon the sun. I was to bear in mind the following requirements: First of all, excellent day seeing; scarcely second to this, excellent night seeing; fitness of site for living conditions, accessibility, availability of power, electric or other, either from abundant water sources or from commercial distribution, etc.

For the purposes of this survey, a 9-inch achromatic objective, focus 108 inches, was kindly loaned to the Committee by the Alvan Clark and Sons Corporation. The Lick Observatory loaned a Warner and Swasey micrometer, a declination axis and slow motion by the same makers, a good centrifugal driving clock, and many smaller pieces of apparatus needed in this work. A prism-and-grating spectroscope was loaned by the Chabot Observatory, and an excellent helioscopic eyepiece was supplied by the Yerkes Observatory.

The mounting of the telescope was made in the Lick Observatory instrument shops from my designs. Its general features will be understood from the accompanying photographs.* It was expected that tests in out of the way places would be required, and on this account the mounting was made as light as was consistent with sufficient rigidity, and the parts were so arranged that they could be transported on pack animals over mountain trails. The tube is constructed of aluminum sheets one sixteenth of an inch thick, suitably strengthened by end and center castings, etc. Its weight, complete with lens, tail piece, micrometer, and counterweights, is only 88 pounds. The wooden polar axis is provided with steel trunnions which turn in roller bearings.

* Not here reproduced.

The object glass is of excellent quality, and the mounting met the requirements in a satisfactory manner.

The equipment had been shipped ahead and was awaiting me in Los Angeles. In accordance with your instructions, I at once arranged for its transfer to Echo mountain, where Professor Larkin very kindly put the resources of the Lowe Observatory at my disposal, in order that comparative tests might be made with the Lowe 16-inch telescope and my instrument.

President H. E. Huntington, of the Pacific Electric Railway Company, through his general manager, Mr. Epes Randolph, extended the courtesies of the Mount Lowe Railroad from Los Angeles during my stay, thus facilitating the work.

Mount Lowe, like its neighbors of the San Gabriel range, rises most abruptly from the Los Angeles plains. One approaches the mountain by a mesa which rises so gradually to the precipitous spurs that he scarcely notices he has left sea level behind. Then a cable incline lifts him suddenly to an altitude of 3,200 feet. At the head of the incline are the power house and other buildings of the electric railroad. Some abandoned chalets stand by the edge of the canyon, whose precipitous eastern walls send back the echoes that give the place its name. Properly, Echo mountain is but a spur of Mount Lowe, which is a vast pile of just such spurs, culminating in a round, rocky, desolate summit, at 5,650 feet above the sea.

The original intention, it is said, was to place the Lowe Observatory on the top of Mount Lowe, but the electric railroad was never completed to the summit, and this plan was not carried out. The present terminus of the road is at Alpine Tavern, 4½ miles from the top of the incline and at 1,100 feet greater elevation.

The Lowe Observatory is situated at Echo mountain, 3,400 feet above the sea, a short walk above the head of the incline, and by the electric road is within fifty minutes of Pasadena and an hour and twenty minutes of Los Angeles.

The mountain rises immediately back of the Observatory, on the north, to an angular altitude of perhaps ten or more degrees, so that there is not a clear horizon in this direction. Toward the northeast, across a canyon, another spur rises to a greater elevation, and here, too, the view is obstructed. In other directions the horizon is clear.

The water supply of the Observatory is derived from a spring in one of the canyons a short distance away. A reservoir has been

constructed just in front of the Observatory and a few feet below. The water from the spring is piped into this, but not into the Observatory, the elevation of the spring being insufficient for this purpose.

Judging by surface indications, Mount Lowe as a whole is not well supplied with water. Nevertheless, in its canyons, even well up toward the top, one may find clumps of bay and sycamore trees, and these are known to require a fairly constant supply of water. It is possible, therefore, that by judiciously tunneling in the vicinity of these trees water could be developed in places where none is now visible on the surface except immediately after rains.

It soon became apparent that the prevailing level of the fog in the Los Angeles region is higher than that for the corresponding season in the neighborhood of San Francisco. It also happened to be the season of maximum drift, namely, April and May. I was therefore detained at Echo mountain beyond the time limit first set for my stay, the fog being around or above me fully two thirds of the time.

For this and other reasons, the suggestion to make tests at two other stations on Mount Lowe was modified, and only one was chosen. This was Inspiration point, at about 4,500 feet altitude, and accessible from Alpine Tavern. Here fog did not trouble, and tests were soon completed, several runs being made by the electric road down to Lowe Observatory for comparative tests with the 16-inch. On the 13th of May the 9-inch was finally dismounted, and the equipment was packed and sent to Los Angeles for shipment to San Diego.

My stay at Mount Lowe had shown that an elevation of 2,500 feet would in this section be entirely insufficient to escape the prevailing height of either the fog or the dust. It had also impressed upon me the enormous advantage of quick and ready communication with prosperous communities, such as the electric road made possible with Pasadena and Los Angeles, and of the electric power which that brought to hand for mechanical work.

I have spoken of the abruptness of the San Gabriel range. The usual mountain masses, however lofty, have a low altitude compared with the measure of their bases. Perhaps an average proportion in the California coast ranges would be that a mountain with an altitude of 1 mile would run a base line 15 miles to reach the last of its foothills. Mount Lowe has no foothills, and falls to the mesa's edge in 2½ miles. Mount Wilson, adjacent on the east, higher and broader, holds all its southern spurs well within a sweep of 4 miles

from the summit. Their slopes are practically unscalable except by constructed trails. Where the steepness does not forbid the way, the chaparral everywhere disputes it. This brushwood growth is peculiarly characteristic of the semi-arid mountains of southern California. Mount Lowe wears this covering of chaparral, but it is nowhere luxuriant. The decomposing white granite is inhospitable soil for vegetable growth. The mountain is sterile and forbidding, except in a few of its canyons. The white granite sand, scarcely covered at all with accumulating humus, stares everywhere through the irregular lanes in the chaparral. In many places the faces of the slopes lie almost bare and glitter unpleasantly in the sun. There is little doubt that radiation from the exposed surfaces accounts for the fact that the day seeing both at Echo mountain and at the higher station at Inspiration point averaged lower than the night seeing, which was, on the whole, excellent.

The term mesa is used in the Southwest to denote the fringe of detritus washed down from the mountains and sloping away from their bases to the plains, or the mesa may be formed, as in the region about Flagstaff, Arizona, by lava flows from volcanic cones; but always, if one examines it as a whole, one finds a sloping upland contiguous on the upper side to mountains and on the lower side to plains or valleys. It is in this sense of contiguous sloping uplands that I use the word in this report.

My departure from Los Angeles for the south was somewhat delayed by the accidental side-tracking of a part of the freight in its shipment down from Echo mountain. In the meantime I made my first trip to Mount Wilson, going up the Sierra Madre trail on the east side, staying over night, and descending the west or Pasadena trail on the second afternoon. Some days previously I had the good fortune to make the acquaintance of Mr. T. P. Lukens, of the United States Forestry Commission. He has been of the greatest assistance to me in furnishing reliable information respecting the mountains of southern California and northern Arizona. He spoke enthusiastically of Mount Wilson and advised me to see it. The only access to this peak at present is by the trails just mentioned. These are only wide enough for pack animals, burros, or mules, and about four hours is required for the ascent or descent. I had been assured that Mount Wilson was very different from Mount Lowe, though from the valley it has the same grim outlines; but I was quite unprepared for the actual view of it. Instead of one barren rock, a succession of rolling knolls forms the summit. The canyons con-

tain spruce and the ridges are covered with chaparral—a growth of Ceanothus (buckthorn), scrub oak, and other evergreen bushes, so luxuriant, so dense, that passage through it is quite impossible without the aid of an ax. This appealed to me at once as an ideal covering, changeless the year around, for the checking of solar radiation.

On exploration, Mount Wilson seemed to have in addition these points of advantage: A water supply remarkable for abundance and nearness to the summit; a small peak adjacent to and above the source of this water, somewhat higher than Wilson's peak, which could be utilized for pressure in a water system; abundance of granite rock, both dark and light, some of it showing excellent cleavage, for building purposes.

Absence of wind, as reported by every one acquainted with the place, is evidenced here by the straightness and symmetry of the trees, one apparent exception being the Ponderosa pine, with a great flat crown as if bent under the pressure of storm and wind. Later, I learned from a paragraph in a report of the United States Forestry Commission that the peculiar shape of this tree is a matter of age and habit and not due to local conditions.

The live oak is everywhere, both as tree and shrub, and in the canyons maple, alder, sycamore, bay, and cottonwood are found. Of the conifers, the big-coned spruce most abounds, and it is to this and the chaparral that the mountain owes its unbroken slopes of green.

In one thing only does Mount Lowe have an advantage, and that is in the possession of an electric road. As to electric power for mechanical and other uses, a line 4 miles in length from Wilson peak would connect with existing systems. An electric railroad could be constructed from Pasadena to the summit of Mount Wilson quite as easily as to the summit of Mount Lowe; or, as an alternative, it would be possible to extend the present railway from Alpine Tavern around Markham and San Gabriel peaks to the summit of Mount Wilson. It is estimated that such an extension would not need to be more than 10 miles in length.

On the evening of May 19 I reached San Diego and occupied the following day with business matters and acquiring information concerning conditions in the back country, especially as to the routes into the mountains. On the 21st I made an early start for the nearest of these, San Miguel, a peak 2,600 feet high, standing conspicuous and alone about 13 miles in an air line east of the town. The trip was made in a buckboard, as far as there was any

road, to the foot of the mountain, and then on horseback to the summit. Clouds lowered all the morning and hid the peak during the approach, but I had a clear and excellent view from the summit. I saw a country very different from the Los Angeles plains. It looks like an old land, and has the simple form and coloring that distinguish the arid regions everywhere. Sharp ridges rise ever more numerous back from the sea, but among the rolling dunes occasional peaks stand alone, boulder-strewn and desolate. San Miguel itself is a type of these—treeless from its top to its base.

The horses picked their way without difficulty through the scanty chaparral. Most of it stood just to their knees, though now and then a thicket of buckthorn rose to the riders' shoulders. From the summit to the base I looked in vain for any sign of springs or live water-courses, and there was almost no trace of animal life.

Here and there in the valleys below shone little reservoirs of water made by damming open streams. Now, at the close of the rainy season, none of these were full. I was told that the great Sweetwater dam, built at a cost of a quarter of a million dollars, has scarcely 10 feet of water behind it. There seems to be no bottom to this land, and in a season of ordinary rainfall most of the water sinks out of sight before it gets to these dams. The higher mountains, with their winter snows, are 40 miles away, and fluming is expensive business.

Riding back to town I read everywhere the story of the land of little rain. Just for a few miles at the foot of San Miguel a grassy mesa rolls irregularly down, but passes soon into the dunes of cactus and prickly pear which extend to the sea. In the little winding valleys orchards are dead or dying. Along the dry bed of the Sweetwater river pumps are trying to recover enough to save the trees of that district.

San Diego is an incorporated city of perhaps 18,000 people.

I went to Lakeside a few days later, and the San Miguel experience was in essentials repeated. Occasional orchards thriving, more abandoned, dying, or dead, according as owners have been able or not to hold out through the dry years and pay the high price of water in a drouth. At Lakeside I took the stage for Cuyamaca, a ride of 35½ miles. This region was chosen as the site of my station for the following reasons: My observations in San Diego had led me to believe that in this region, as at Echo mountain, any altitude below 4,000 feet would often be covered by fog at this season. On this account, as well as on account of the apparent lack of water within

1,500 feet of the summit, the establishing of a station on San Miguel seemed inadvisable. Of the higher mountains, Cuyamaca and Palomar seemed to promise most, but in San Diego I was able to get little definite information as to the latter. The impression prevailed that it was very inaccessible. Moreover, Cuyamaca was one of the mountains suggested by your Commission as worthy of exploration, and it has, besides, a daily mail stage from Lakeside to the Stonewall mine at Cuyamaca lake. The latter is at an elevation of about 4,500 feet, and there it seemed most expedient to make my first camp.

In San Diego I had received much valuable assistance from Mr. Ford A. Carpenter, local forecast official of the United States Weather Bureau. Through his interest I became acquainted with Mr. M. C. Healion, president of the San Diego Flume Company. This company controls the water from Cuyamaca lake, and has a house there. President Healion very kindly placed a room at my disposal, and his foreman in charge proved a very helpful assistant.

Cuyamaca is an Indian word, said to mean "cradle of the rains," and here is the heaviest rainfall in southern California. The lake is 2 miles long and less than 1 mile in width. The three Cuyamaca peaks rise about it—South peak to 6,500 feet, and Middle and North peaks to approximately 6,000 feet. Rattlesnake hill and a low circle of chaparral-covered hills shut off the view of the Colorado desert, whose first sentinel peak, just visible from the middle of the lake, is barely 6 miles distant in an air line, for Cuyamaca drops abruptly on the east. This nearness of the desert augured ill, and indeed the pull between the great oven and the sea kept the winds at work. The telescope was erected on the green slope at the base of Middle peak, near the dam and the west shore of the lake.

Grass is abundant here. Forests of oak rise back of the meadows, and above them the fir, the cedar, and the pine, in turn. The summits are forbidding, and their ascent with horses is impracticable unless road or trail has been cut. Up South peak such a road was made a few years ago by the United States Geological Survey, and that ascent is now easy. The view from this summit, supplemented by constant reference to the topographical map prepared by the United States Geological Survey, gave me an excellent idea of the entire surrounding country. To the north lay Palomar, beyond which rose the white cap of Old Baldy in the San Gabriel range, 110 miles away. Thence the San Bernardino and San Jacinto mountains lead toward Cuyamaca and the connecting ranges that

drop to the great desert on their eastern slopes. The Laguna mountains reach up from Mexico and thrust out a shoulder that just intercepts the view of the great Colorado floor stretching east to Yuma. Down to the west and south fall the lesser hills to the sand dunes and the sea, a country all creased and crumpled, arid and brown. Just below Cuyamaca lie Viejas and Elcajon, peaks whose altitudes and situations had appealed to me on the map as promising sites to consider. The field glasses bring them within a mile or two, and I find them, as I feared, exaggerated San Miguels; Elcajon more precipitous, Viejas more barren, equally treeless, and by all reports equally destitute of springs above their bases.

I learned that frosts may occur any month in the year at the lake; that the thermometer may rise above 100° in summer and fall below zero in winter; that the winds blow very nearly all the time, and that the lake dries up in summer.

The seeing at Cuyamaca proved to be what one might expect in a region of high winds and rapidly changing temperatures, and all that could be learned by inquiry and inspection failed to indicate anything more promising in this extreme southern end of the state, excepting Palomar only.

I had been told that one must return to San Diego, go by rail to Escondido, and thence by stage in order to reach Palomar. But I had looked across to it repeatedly from both North and South Cuyamaca peaks, and had concluded that one should be able to reach it on horseback from Cuyamaca, through Julian and either the Santa Ysabel or San Felipe valley into Warner's Ranch valley, which is at the southeastern base of Palomar. I set out, therefore, on the morning of the 6th of June. As I rounded the lake and turned northward I encountered light breezes from the desert. The peculiar subtraction of vital force effected by these desert winds is scarcely to be understood, but it is never to be denied by those who have felt it.

After a hard, all day's ride of about 40 miles via Santa Ysabel valley, through canyons, over hills, and across valleys, traversing roads at times remarkable for steepness, I reached Cook's ranch, on a shoulder of Palomar, late in the evening. I made excursions over the mountain on the following day, and the next day returned to Cuyamaca.

Nothing prepares one for the surprise of Palomar. There it stands, a hanging garden above the arid lands. Springs of water burst out of the hillsides and cross the roads in rivulets. The road is through forests that a king might covet—oak and cedar and stately

fir. A valley where the cattle stand knee deep in grass has on one side a line of hills as desolate as Nevada; on the other side majestic slopes of pines.

Among the possible places for our purposes, a "bench" on Mr. Cook's ranch seemed especially attractive and caused me to consider most carefully the question whether it would be advisable to bring the equipment to Palomar. Your Committee has had in my reports descriptions of this place in some detail, and a balancing of the advantages and disadvantages of this site, the most promising in the San Diego section, as against Mount Wilson, the most promising in the Los Angeles region. This bench is situated at the southern edge of the broad, rolling, open space known as Dyche valley. Its contours adapt it admirably for all the requirements of an observatory site. Covered as it is with a good stand of maturing grain and fringed with magnificent oaks, its appearance is most inviting. Springs of water are found in nearby knolls above it, and a hundred feet or so below its edge an abundant stream breaks out that is said to run without change the year round. However, with the long California summer, no amount of irrigation could control the wide pasture lands and secure the changeless green surface of Mount Wilson. A few miles to the east, and less than 2,000 feet below, stretches Valle de San Jose, which heats like an oven and leads away toward the desert. The east winds in winter are said to be furious and not infrequent. The summer climate is almost uniformly pleasant, and the nights are always cool. The school session is a summer one, for snows in winter may block the roads and render them impassable for children. On the days that I was there the sea breeze was perceptible as early, certainly, as six in the morning, and it grew strong by noon. However, the bench I have described seemed especially sheltered by neighboring hills on the northwest and on the east, and its trees showed very little effect of wind. As soon as the sun was down, a decided chill was noticeable, and though the days had been warm, Mr. Cook proceeded, as a matter of course, to build a fire. The dews are heavy here, as at Cuyamaca. These facts were not favorable to one's expectation of the best seeing. The remarkable stillness, the steady temperature, and the evergreen covering of Mount Wilson could not be found on Palomar, though my judgment, from this cursory examination, would lead me to expect steadier atmospheric conditions here than at Cuyamaca.

There remains one other important factor against Palomar as a site for an observing station—its extreme isolation. Escondido is the nearest town of any pretensions, and that is 35 miles away. There is a road down the western side of the mountain, laid out to be a 10 per cent grade, but constructed steeper in places. A road is contemplated down the southeast end of the mountain, from Mendenhall valley into Valle de San Jose. This has been surveyed and, I was informed, the construction ordered. Whether railroads will soon come nearer the mountain's base than they are at present is entirely uncertain.

There are enormous disadvantages in the way of developing the country back of San Diego because of its remoteness, its surface configuration, and the insufficiency of water. It seems to me improbable that the power, the accessibility, the contact with civilization, and the resources of a city like Los Angeles, which Mount Wilson has at hand, will ever come within reach of Palomar. Moreover, its nearness to the desert and the absence of protecting mountains to shield it from the winds that play between the heated interior and the sea make it far from probable that we should have here the equable conditions required for the highest grade of astronomical work.

On returning to Cuyamaca I found that the equipment previously packed was already on the way to San Diego. On arriving there I received instructions to return to the Los Angeles region. The equipment was shipped at once to Pasadena and sent by pack train up the Sierra Madre trail to the summit of Wilson peak.

At the close of two weeks' tests at this station a week was spent at Flagstaff, Arizona, before returning north, where, by the courtesy of Mr. Percival Lowell, the records and instruments of the Lowell Observatory were freely put at my disposal to gain a knowledge of the conditions in that section.

The 9-inch telescope has been used at the following stations: Mount Hamilton, Echo mountain, and Inspiration point on Mount Lowe, Cuyamaca in San Diego county, and Mount Wilson.

The instrument was set up on Mount Hamilton as soon as completed for the purpose of testing the lens and mechanical parts of the mounting. Comparative observations were made with the 9-inch, 12-inch, and 36-inch telescopes. Some observations of the sun were made, but most of the tests consisted of observations of the stars, and particularly of close double stars.

It is well known that the maximum efficiency of a very large telescope can be secured only under excellent atmospheric conditions. It is also true that a large aperture is required for the most critical differentiation of the various qualities of seeing. It may very well happen that what may appear to be very good seeing with a 6-inch telescope will not prove so with an instrument of the largest dimensions. Moreover, different kinds of work vary in their requirements. For example, with a comparatively short photographic telescope no perceptible difference in the results will be found on nights of excellent definition and on those when the seeing is only fairly good. Again, for meridian-circle work the most essential condition is a steady image, and for difficult double-star work there is the additional requirement of fine definition.

Even though some classes of work may be carried on very successfully under circumstances which are not altogether favorable, it is nevertheless true that they would be more easy of accomplishment under excellent conditions; and it is also true that many important investigations require the highest obtainable efficiency of the most powerful telescopes for their successful prosecution. That site, therefore, which affords excellent seeing the most continuously, and the necessities, conveniences, and comforts of life the most abundantly, will be the best adapted for the needs of a great observatory.

For several years I have used the 36-inch and 12-inch telescopes of the Lick Observatory for double-star and other observational work, and on many occasions when working with one instrument I have gone to the other in order to make a comparison of the two. Whenever the seeing appears excellent with the large telescope it also appears excellent with the smaller one; but when it appears only fairly good with the 36-inch there is a tendency to rate it somewhat higher with the 12-inch. As soon as the tests began with the 9-inch it was further noted that there is a very perceptible difference between it and the 12-inch in the same direction. It was my especial object while the instrument was set up at Mount Hamilton to become acquainted with the characteristics of the image formed with the 9-inch under varying conditions, in order that I might properly interpret them and retain as far as possible 36-inch standards of excellence while making the tests in the various localities. To have proceeded in the opposite direction, by capping down the larger instruments to a 9-inch aperture would, in my opinion, have resulted in a lowering of the standard which it is

eminently desirable to maintain. It is manifestly impossible to give a numerical rating of the seeing which shall indicate its characteristics fully and without ambiguity. Nevertheless, it is convenient to use a number to express one's estimate of the effect of atmospheric conditions taken as a whole. The scale which I have used in this way embraces the numbers 1 to 5, inclusive.

The number 5 is used to denote seeing perfect in every way, a standard of excellence that seldom obtains; 4, excellent seeing with the excellent conditions lasting for considerable intervals; 3, good seeing, but with the images less sharply defined than indicated by 4, or having such conditions lasting for much shorter intervals; 2, poor seeing, images unsteady, large, or blurred, yet of such quality that good work of some kinds can be done, but not that requiring fine definition; 1, seeing so bad that good work with large instruments is out of the question.

At Lowe Observatory the 9-inch telescope was erected on April 23, between the observatory and the reservoir, and was dismounted on May 2. Observations of the sun were made during the forenoon, and tests of seeing at night—till midnight or later. On one forenoon only was the day seeing excellent. The first observations were made shortly after 6 a. m. At that time the sun's limb was sharply defined, and the granulations of the surface very clear and distinct. Several groups of sun spots were visible, and much detail could be clearly seen in both umbræ and penumbræ. At the time of the earliest observation the sun's image was remarkably free from passing heat waves, but this condition did not continue throughout the forenoon. The seeing gradually became worse, and by noon it was bad. It was the usual experience here to find the day seeing grow worse as the forenoon advanced, probably owing to radiation from the nearby and but scantily covered slopes of the mountain.

The night conditions at Lowe Observatory were on the average very much better than those prevailing during the day. Tests were made on all clear nights. Several were found to be excellent, others good, and only two bad, one of these being the night after leaving Inspiration point, when a violent storm was coming on.

In order to familiarize myself with 16-inch telescope conditions, I employed that instrument on parts of five nights in looking for new double stars. On each of these nights tests were made also with the 9-inch telescope. The following pairs, thought to be new, were found:

DM + 48°	1707	8h 49m	16s.3	+ 48° 35′.9	0.9	10 and 10, Comp. of $0\Sigma 196$		
48	1716	8 53	16.6	+ 48 14 .5	0.3	8.5	8.8	
50	1605	8 54	48.1	+ 50 28 .6	3.0	9.1.....	....	
51	1482	8 56	25.6	+ 51 12 .7	0.4	8.8	9.0	
50	2174	15 19	25.3	+ 50 2 .2	3	8.2	12	
50	2178	15 23	18.8	+ 50 49 .7	1	7.3	12	
49	2408	15 31	40.2	+ 49 16 .9	0.4	8.2	8.6	
51	2030	15 42	21.5	+ 51 14 .3	0.3	8.5	8.5	
51	2077	16 15	57.9	+ 51 54 .7	3.0	8.9.....	.....	.....
51	2105	16 24	22.1	+ 51 54 .1	3	7.3	12	
51	2106	16 25	1.2	+ 51 45 .4	4	6.2	13	
51	2130	16 40	10.9	+ 51 48 .7	¼	7.5	7.5	

I have not secured measures of these pairs. The first on the list is of special interest on account of its being the companion of a well-known third-magnitude double star, viz., 9 τ Ursæ Majoris, or $0\Sigma 196$.

Alpine Tavern is situated among the trees near the head of a canyon. In its immediate vicinity there is no place that commands a clear horizon. A broad trail leads from it to Inspiration point, and narrow trails run up the mountain to the summit. No pack animals are kept at Alpine Tavern, and on this account it was not feasible to take the equipment to the summit of Mount Lowe. The only place within reach of the tavern that seemed suitable for the erection of the telescope was Inspiration point. The first observations were made here on May 7 and the last observations were made May 13, just before the telescope was dismounted.

At Inspiration point observations of the sun were made at intervals throughout the forenoon and the earlier portions of the afternoon, the first ones usually about 6.30 a. m.

It was found that the day seeing here is similar to that at the Lowe Observatory. Early in the morning the seeing was sometimes good, but it never had that excellent quality which was noted on one of the mornings at Echo mountain. During the forenoon the seeing always became worse, and in the afternoon it was never good. The mountain side to the east of this station was burned over only a few years ago, and it has not become covered with chaparral again. There is much surface exposed, and it is probable that the radiation from this is one of the causes of the seeing becoming worse as the day advanced.

Tests of the night seeing were also made, but they were not continued later than midnight. Some of the nights were excellent, others were fair, and none of them were very bad. It was possible

on three of the nights to take the car to Echo mountain and make additional tests with the 16-inch telescope.

The top of the Incline and the Lowe Observatory can be seen from Inspiration point. During my stay at the higher station I was constantly above the fogs which had hindered me so much at Echo mountain. They did not, during this period, rise to my elevation, 4,500 feet, but very often did cover Lowe Observatory all day long.

The telescope was erected at Cuyamaca on May 27, and taken down on the afternoon of June 5. Observations were made at intervals during the day, from shortly after six o'clock in the morning till the middle of the afternoon, and then again during the earlier portion of the nights. The results were not at all favorable. The seeing was nearly always poor, occasionally good, but never excellent.

The San Diego Flume Company has kept a record of the weather conditions at Cuyamaca dam almost continuously since 1888. I made some examination of this record, and from it derived considerable information respecting the weather conditions which prevail in this region.

The readings of the maximum and minimum thermometers show that the daily range of temperature is usually large, and that the weather sometimes becomes very cold in winter and very hot in summer. In winter the temperature occasionally falls several degrees below zero, and in summer it may rise above 100°. Thus, in June, 1899, there were five successive days with maximum temperatures of 104, 108, 110, 105, and 101. The minimum temperatures for the same days were 50, 46, 55, 55, and 54 degrees. The highest temperature noted in the record is 113 degrees.

The rainfall is heavier at Cuyamaca than at any other place in San Diego county. The heaviest snows also occur here upon the peaks, sometimes amounting to a depth of several feet. Even about the lake, in the little valley among the peaks, the snows are heavy. At one time last winter 30 inches lay upon the ground for six weeks, and ice formed on the lake to a depth of 8 inches. Here, as in other regions adjacent to the desert, water cannot always be counted upon from the melting of the snow. A warm dry wind may lick up the moisture and carry it away, leaving scarcely a trace.

The heaviest rains at Cuyamaca come in winter, but there are also local thunder showers in summer. During the last days of my stay in this region, I saw at a distance a number of these showers. It is said, however, that they are sometimes productive of very heavy rains. The prevalence of cloudiness in connection with these

showers and the disturbed atmospheric conditions indicated would without doubt prove an obstacle to the prosecution of solar work in this region.

The thermographic record obtained by the expedition at Cuyamaca extends over twelve days, from May 27 to June 8, inclusive. The temperature curve for this period is generally quite smooth and free from rapid alternations, but has, as a rule, a large daily range. This sometimes amounts to as much as 35°, passing in one instance from 46° at 6 a. m. to 81° at 4 p. m., and in another from 43° at 5 a. m. to 78° at 5 p. m. It was particularly noted that the only times of constant temperature were those when a strong sea breeze was blowing. Nevertheless, at such times, the seeing was invariably poor. The temperature curve at night was generally steep, often having a declining rate of from 3 to 4 degrees an hour. Only once was there a period of constant temperature at night. This lasted about four hours, with a variation of about 1°.

A thermograph was placed on North Cuyamaca peak on May 29, and allowed to remain there in a suitable shelter until June 8, thus giving a record for ten days. This record shows that more frequent small alternations of temperature were taking place on the summit than at the lower station, producing a less regular curve, especially during the day. The daily range in temperature on the summit was, however, much smaller. As a rule, it was less than 20°; the maximum was 29°. The night temperatures on the summit were quite satisfactory. On most of the nights there were intervals of several hours during which the temperature remained nearly constant or fell very slowly.

During the time I was at Cuyamaca the dews were heavy. This is a normal summer condition, due to the rapid fall of temperature at night.

The observations at Echo mountain and Inspiration point on Mount Lowe had shown that excellent night seeing prevails there. The day seeing there was also good at times, but it did not have that constant excellence which was deemed desirable.

Mount Wilson is only 3 miles east of Mount Lowe, and there was every reason to think that it has the same excellent night conditions that prevail on Mount Lowe. On the other hand, it appeared probable that it would have better day conditions, owing to the control of insolation and radiation by its dense covering of evergreen trees and chaparral. On going to Mount Wilson, therefore, I was instructed to devote the greater portion of the time to testing the see-

ing during the day. As will be seen from the details given below, the day conditions were found to be excellent; very much better than at any station tested in the south. Following is a record of tests made:

Thursday, June 18.—The erection of the telescope was completed this afternoon, but not in time to make any observations on the sun. Tests were made on the stars during the earlier portions of the night. The seeing was only fair, about 3.

Friday, June 19.—No tests were made during the forenoon. Tests made upon the sun during the afternoon showed the seeing to be good until 5.30 p. m., when observations were discontinued.

Saturday, June 20.—Tests were made on the sun at intervals from about 8.30 in the morning until nearly 6 in the afternoon. The seeing was fair in the morning, good in the early afternoon, and very good between 4 and 5 o'clock in the afternoon. Even at 6 p. m. it was nearly as good as the best day seeing we had at Cuyamaca. Very little wind.

Sunday, June 21.—Tests were made at frequent intervals throughout the day, beginning at 6.20 a. m. During the forenoon a pronounced sea breeze was blowing, the air had a chilly feeling, and the seeing was bad. In the afternoon the wind had decreased to a very light breeze, and the seeing was greatly improved. The sun spots have today been undergoing rapid changes. Some of these changes could be appreciated without difficulty or uncertainty after an interval of only a few minutes. From 3 to 4 o'clock the seeing was at its best. The granulations of the surface, the detail in the spots—both in umbræ and penumbræ—were then very clear and steady. The appearance at this time may best be described by saying that the effect as to clearness, sharpness, and steadiness was very like that of a print from a steel engraving.

At 4.30 the seeing was not so good as between 3 and 4 o'clock, but still the details in the umbræ and penumbræ of the spots, the filamentary character of the umbræ and the granulations of the sun's surface were very distinct, and the periods of steadiness lasted for considerable intervals. Between 5.30 and 5.45 the seeing was not so good as earlier in the afternoon. The image was somewhat less steady, and occasionally considerably blurred.

No observations were made at night. There was a light wind at Martin's camp during the evening, probably less than 10 miles an hour.

Monday, June 22.—The sun was examined from 6.30 to 7.15 a. m., and the seeing was found to be poor. A pretty strong west wind was blowing. During the forenoon the sky clouded and it remained cloudy until late in the afternoon. Unquestionably a storm condition prevailed during the day, and no doubt its effects lasted into the night.

From 8 to 10 p. m. the seeing was fair, about 3. The stellar images were small and distinct, but with considerable motion and more or less continual breaking of the diffraction pattern. A strong northeast wind, perhaps 20 miles an hour, was blowing during the night.

Tuesday, June 23.—From 3 a. m. to 4.15—*i. e.*, dawn—the stellar images were much better than in the earlier portion of the night. The central disc of the image was small, distinct, and sharply defined, and the diffraction pattern was much more satisfactory.

Very early in the morning the wind went down almost completely, and when the sun was first examined, from 7 to 7.20 a. m., the image was found to be excellent. The granulations of the surface and the structure of the spots were very clearly defined and almost wholly free from blurring.

By 10.30 a. m. the seeing had gone to pieces. While the image was occasionally clear and distinct, this was by no means the normal condition. On the contrary, it was during the greater part of the time more or less completely blurred.

Between 11 and 12.10 the seeing was considerably improved. There were intervals when the definition was good, but longer periods when it was poor, and occasionally the blurring was bad.

Between 2 and 3 o'clock the seeing was poor. It clouded up about the middle of the afternoon and remained cloudy until shortly after dark. It then became beautifully clear. During the night there was a strong wind from the northwest.

Wednesday, June 24.—Early in the morning, 6 to 7.30, the seeing was not very good, nor was it very bad. There was more or less constant haziness without pronounced blurring. Much detail could be seen, but not very satisfactorily. It did not have the steel plate effect of the finest definition. From 9.30 to 11.45 the seeing was poor. In the afternoon it was fair between 4 and 5 o'clock and somewhat worse during the next hour.

In the evening, from 8 to 10.30, the seeing was good—3 to 4. This was also the case during the latter part of the night—*i. e.*, from 3 to 4.15 a. m.

Thursday, June 25.—Between 6.30 and 7.15 a. m. the seeing was excellent in every way, the image very clearly defined and almost wholly free from blurring—steel plate effect.

Between 9.30 a. m. and 12 m. the image was less satisfactory than early in the morning. The limb of the sun was not so steady, and blurring was more frequent and more pronounced.

In the afternoon, between 4 and 5.45, the seeing was good, but by no means equal to that which we had early in the morning. Professors Campbell and Hale arrived at Wilson peak this afternoon. Professor Hale rated the afternoon seeing at 4.

Friday, June 26.—Early in the morning, from 6.15 to 7.15, the seeing was very good, but not equal to that of yesterday morning, nor to that of last Sunday afternoon. Between 9 and noon it was less satisfactory. It was then fair and was given a rating of 3.

During the middle portion of the day it was very warm. No observations were made in the afternoon until about 4 o'clock. The seeing was then excellent. Professor Hale rated it at 4. It continued good until nearly 6 o'clock.

In the early part of the night the seeing was good, but not excellent. The central discs of the star images were small and distinct, but the diffraction patterns were more or less in motion.

Saturday, June 27.—Early in the morning, between 6 and 7.15, the seeing was excellent. Later, during the afternoon, it was fair. In the latter part of the afternoon it was found to be excellent again.

Sunday, June 28.—Greater part of the day spent in exploring the mountain in company with Professors Hale and Campbell, and Messrs. Staats, Holmes, and Lukens, of Pasadena.

About 8 o'clock the seeing was found to be very good, and it was also good in the middle of the afternoon.

Monday, June 29.—Partially cloudy most of the day. No observations made during the forenoon. At 3.30 p. m. the seeing was not good.

Tuesday, June 30.—Seeing good in the morning and also in the afternoon. Most of the day spent in photographic work. Seeing good in the evening.

Wednesday, July 1.—No observations were made during the forenoon. The seeing was excellent between 4 and 5 o'clock in the afternoon. In the evening a wind arose and the seeing went to pieces.

Thursday, July 2.—At 9 a. m. the seeing was good. There was some blurring and a little disturbance at the sun's edge, but these

were not very pronounced. The seeing was rated at 3. Telescope was dismounted during the forenoon.

The summit of Mount Wilson consists of perhaps 200 acres of more or less rolling ground, intersected by ravines of moderate depth, sparsely covered with splendid pines, with open grassy spaces of considerable extent, affording ample and excellent sites for buildings, large or small, with sufficient room to meet the needs of a large community. The summit proper is situated near the southwest point of this area. Here, in the vicinity of the old building known as the Casino, is, in my opinion, the best site for observatory purposes.

To the southwest of the Casino the ground drops away rapidly and is covered as far as one can see down into the canyon with a dense growth of chaparral, giving an evergreen covering that effectively shades the ground at all times. The spurs to the southeast of the summit are also largely covered with a growth of chaparral and in places also with a larger growth of oak and pine. In this direction, however, the covering is not always so complete as toward the southwest. This is due to the more precipitous character of the mountain side in this direction, making it less easy for vegetation to gain and retain a foothold. Nevertheless the whole barren areas are of comparatively small extent, and this is especially so in the vicinity of the Casino. Moreover, the barren areas near the Casino are not generally solid rock, but slides of decomposed granite, and over these slides it would be possible with intelligent care to foster a growth of chaparral which would eventually more or less completely cover them.

In this connection it is well to hold in mind the abundant supply of water that is within reach at Strain's camp, only a third of a mile north of the Casino and less than 300 feet lower. Here are wells which at the present time are constantly overflowing. Even after a series of dry seasons it was found that the upper of these wells alone would by pumping yield a constant supply of not less than 25 gallons a minute. This would be sufficient for the scientific and domestic purposes of a large observatory, and also for irrigation purposes, should irrigation ever be found desirable. Considering their nearness to the summit and the difficulties that are ordinarily encountered in finding water near the tops of mountains in southern California, these wells must be regarded as little short of phenomenal. If Mount Wilson should be selected as the site for an observatory, it would be eminently desirable that the Observatory

should control at least one of these wells and its immediate watershed.

It is not known how difficult it would be to obtain a large supply of water at any other place near the summit of Mount Wilson. It is certain that wells or tunnels at many places would prove failures, as did the first one that was designed to furnish a supply of water for Martin's camp. Martin's camp is, in an air line, about half a mile south of the summit, and 700 or 800 feet lower. It is now supplied by a tunnel on the west side of the mountain at a distance of perhaps 500 feet in altitude below the summit. It is possible that the resources of this tunnel might be materially increased by extending it further into the mountain, or by running drifts parallel to the mountain side. This, however, is by no means certain. The result of further tunneling would depend almost entirely upon the character and arrangement of the strata encountered in making these extensions, and what these would be in a granite mountain densely covered with brush is not easily foretold.

As it stands today, the tunnel that supplies Martin's camp does not seem to afford a sufficient supply of water for a large community, such as might eventually grow up around an important observatory, and it certainly would not furnish the abundant supply that would enable water to be used extensively for irrigation, which would be necessary, for a time at least, if an attempt were made to clothe the barren slides with a covering of chaparral. The greater distance of the present tunnel below the summit as compared with the wells at Strain's camp is a forceful argument in favor of the latter.

An undulating ridge connects Mount Wilson with San Gabriel peak. Upon this ridge, at a distance of perhaps half a mile from the Casino, is a summit locally known as Alta, which rises about 60 feet higher than Mount Wilson itself. Alta is not of large dimensions. If an observatory were placed upon Mount Wilson it would be the most favorable site available for the storage of water. The pressure obtained from a reservoir placed here would not be very great, but would be enough to make it of extreme importance as a protection against fires and as a distributing center, etc.

While in southern California, from April 17 to July 19 (with the exception of the interval from July 8 to 16, when I was in Arizona), I watched the drift of ocean fog over the land in reference to its bearing upon the height at which an observatory would have to be placed in order to be above its prevailing higher levels.

In the Los Angeles region it soon became apparent that the height of the fog at this season varies greatly on different days. One morning it will be lying almost in contact with the ground and not advancing far from the sea. More frequently, however, the upper level has an elevation of from 3,000 to 4,000 feet, only occasionally exceeding the latter altitude.

At Echo mountain the upper surface of the fog was sometimes just below the Lowe Observatory. Whenever it was very near, the seeing seemed to be unfavorably affected. When it was a thousand feet or more below, no manifest influence was noted, either for good or bad.

The same characteristic was noted at Inspiration point. Here, also, when the fog rose in the canyons to an elevation nearly equal to that of my station, the seeing appeared to be unfavorably affected.

Mount Wilson is somewhat protected from the direct drift of the ocean fog by a spur of Mount Lowe, and during my stay at Mount Wilson the fog did not at any time approach very near the summit. At night currents of air were noticed descending the sides of the mountain, and no doubt there were ascending currents during the day. No ill effects from them were noticed.

In the San Diego region, near the sea, fog is, according to the United States Weather Bureau record, of daily occurrence during most of the year. In San Diego, in summer, it is generally cloudy at 5 a. m, and often so at 5 p. m. The middle portion of the day is usually clear, or partially so. The record indicates less fog in winter than in summer. The record does not contain any information respecting the altitude of the fogs. I was in San Diego county twenty-five days, from May 19 to June 13, at a season when fog is said to prevail to its greatest extent. On fifteen of these days my observations showed that the summit of San Miguel, whose height is 2,600 feet, was either below or in fog or cloud a portion or all of the day. On three days it was clear at the summit. On the remaining seven days I was not where the peak could be observed.

At Cuyamaca the ocean fog, blown in by strong westerly winds, reached my level on two occasions, and I several times saw it to the west, covering all the peaks in that direction lower than 3,500 feet.

The prevalence of high fogs in these regions makes it evident that an observatory should be placed at an elevation of more than 4,000 feet.

Many of the days and nights at Mount Wilson are nearly windless. Strong gales do sometimes blow; some violent storms may be

expected every year, especially in winter ; but they are not numerous and are to be regarded as exceptional.

Altogether, I spent forty six days on these mountains. In that time there was one severe storm, when the wind was high—probably as much as 60 miles an hour. In addition to this, there were four or five other nights when the wind velocity rose to 25 or 30 miles an hour. Otherwise, the days and nights were very quiet. From all that I could learn, it appears that this record is not more favorable than the average.

Mount Wilson and Mount Lowe are on the southern edge of the San Gabriel mountains, and near the middle of the range from east to west. Here the range has its greatest breadth but not its greatest height. To the northwest of Mount Wilson there is a succession of ridges and peaks that rise to elevations of from 5,000 to 6,000 feet. But it is to the north, northeast, and east that the highest peaks of the range are situated. In these directions, beyond the San Gabriel river, numerous ridges may be seen, some more than 7,000 feet high, with peaks that tower above them, culminating in San Antonio (Old Baldy), 10,080 feet above the sea. These higher ridges lie directly between Mount Wilson and the Mohave desert. By reason of their situation and height they no doubt greatly protect Mount Wilson from winds that would otherwise blow across this region between the desert and the sea.

These higher peaks and ridges are from 10 to 25 miles away. Deep, broad canyons lie between, winding in and out among the mountains until they reach the valley on the south. The advantages of this arrangement from an astronomical standpoint will be readily understood. Streams of cold air settling down from these higher peaks cannot flood Mount Wilson. This source of bad seeing, therefore, appears to be wholly absent. San Gabriel peak is somewhat higher than Mount Wilson, but there is a deep col between them. Flooding with cold air appears to be a fruitful cause of bad seeing at some observatories. Thus Mr. A. E. Douglass speaks of the condition at Arequipa, Peru :

"The observatory is situated close to a river, down which on clear nights a swift stream of cold air descends. When this cold air reached the telescope, the seeing was immediately ruined. When this current was once established, no more good seeing could be expected for the remainder of the night." *

At Flagstaff also the temperature often falls suddenly in the latter

*American Met. Journal, vol. II, p. 395.

part of the night, and at such times the seeing is said to go to pieces.

The San Gabriel and San Bernardino mountains trend nearly east and west, and were it not for the depression between them at Cajon canyon, through which the Santa Fe Railroad passes, they would form a continuous range. The highest points in the San Bernardino mountains, viz., San Bernardino peak (10,630 feet) and San Gorgonio mountain (11,485 feet), are about 40 miles southeast of Cajon canyon. From these high peaks this range dips down toward Cajon canyon, which itself has an elevation of about 2,800 feet above the sea. The main portion of the Mohave desert lies north and northeast of the San Bernardino range. In the vicinity of Cajon pass it has an elevation of about 3,200 feet. Over the desert the air is subject to large and rapid variations of temperature; over the sea, only 70 miles away, it is not. The result of this is a movement of the air which follows the line of least resistance. This happens to be Cajon pass, noted in southern California as a very windy place. When the winds from the desert are strong they sweep violently through this pass southward toward the sea, in the direction of the Santa Ana mountains, and there the Santa Ana canyon affords a passage for them. It, too, is noted for its winds—so much so that in this section of the State any very strong wind is called a Santa Ana. An inspection of the map will show Mount Wilson far to one side of this usual track of strong winds. It is only when the pressure over the desert becomes so great that the volume of moving air cannot be passed through the dip in the range at Cajon canyon that the winds come across the mountains directly and sweep over Mount Wilson from the north.

Only a small portion of the Mohave desert lies directly north of the San Gabriel range. It sometimes happens that winds originating here sweep around the western end of the range through Soledad pass and thence southward through La Cañada. These winds strike Pasadena, Mount Lowe, and Mount Wilson from the northwest. The Southern Pacific Railway from Tehachapi to Los Angeles passes through Soledad pass.

The direction of the sea breeze through the San Gabriel valley is of course from west to east. When it is strong and meets a lighter current coming through Cajon canyon from the Mohave desert, it prevails and swings the latter around the San Bernardino mountains, and both then travel on through the San Gorgonio pass into the Colorado desert.

The sea breeze in the San Gabriel valley is usually strong and is felt almost every day during summer. But, quite unexpectedly, on Mount Lowe and Mount Wilson, overlooking this valley, it is always very light. On almost all of the days I spent on these mountains there were sea breezes, very gentle, fresh, but not cold. At such an elevation, with the ocean only a few miles away to the south, and nothing but low hills between, I should have expected much stronger winds from this quarter. The only suggestion I have to offer as a possible explanation is that the mountains to the west and northwest may be a sufficiently diverting barrier to give these winds a course that passes clear of Mount Wilson, and that the higher ridges to the northeast of Mount Wilson form a barrier to currents that otherwise would pass directly to the desert from the sea. It is 80 miles from Mount Wilson westward to the ocean. In this direction protection is afforded by the Verdugo and Santa Monica mountains, which, though lower than the San Gabriel range, are nevertheless sufficiently high to have considerable influence upon the winds. Moreover, it is probable that they help to divert west winds toward the south, causing them to pass to one side of Mount Wilson.

To the west-northwest of Mount Wilson there are many mountains, reaching all the way to Point Conception, 150 miles away; and to the northwest, further inland, are still more extensive ranges. It cannot be doubted that these ranges have an influence on the winds that come from their direction, tempering, deflecting, or even turning them completely aside. In any event, it is pretty certain that they would find an easier passage from the sea to the desert than by passing over the San Gabriel range.

In this connection we may consider the dust in the air over the Los Angeles plains, for this is very intimately connected with the winds that blow from the Mohave desert to the sea. When the desert is dry—as it is during much of the year, and particularly in summer—and strong winds blow over it, clouds of dust are raised high into the air. When this dust laden air passes through Cajon canyon and out to the sea, the air over the Los Angeles plains becomes charged with dust. In general, the dust filled region of the air has a clearly defined upper limit, here called the dust line, having an elevation of from 3,000 to 5,000 feet, sometimes falling lower, at others rising higher, according to the force and height of the winds. The air above the dust line is not entirely free from dust. Here, as in most other places, there is some dust even at very great elevations. Perhaps no place of moderate height in the temperate zones can be found that is wholly free from it.

During my stay in the San Gabriel mountains I watched the dust line closely. It often rose higher than I was accustomed to see it near the coast in central California. It became evident that an observatory at an elevation of less than 4,000 feet would often be below it, but that it does not usually rise much higher than this. Exceptions, however, do occur. At times it rises to 6,000 feet or more. These appear to correspond to the times when great volumes of air are moving from the desert to the sea, volumes so great that they cannot be passed through the dip in the range at Cajon canyon, and must perforce go over the mountains. This happened once during my stay at Mount Wilson. After some hot days in the valley a strong wind came over the mountain from the north, and the dust line, which before had been high, now rose above the peaks in the immediate vicinity. This lasted for a few days, and then the dust cleared from the valley, and the houses in it were again easily in view. Never a day passed while I was there that Grayback and San Gorgonio, the highest peaks in the San Bernardino range, were not visible. Again I could see without difficulty the lower mountains to the southeast—Palomar, the three Cuyamaca peaks, and their lower neighbors, some of them fully 120 miles away. Views such as this are not exceptional. There are some days when the dust rises high and thick, but according to the most reliable information, this does not happen many times in the year, and it certainly did not happen during my stay.

The summit of Mount Wilson is situated in the southwest quarter of section 29, T. 2 N., R. 11 W., San Bernardino base line and meridian. This section is at present owned by the Pasadena and Mount Wilson Toll Road Company, of which corporation Mr. Wm. R. Staats and Mr. J. H. Holmes, both of Pasadena, at present hold a majority of the stock. This company also owns the west half of the southeast quarter of section 30, and the west half of the northwest quarter of section 32, these being the sections which join section 29 on the west and south respectively. This company also owns the toll trail from the foot of Eaton canyon to the summit. All other lands in the immediate vicinity of the summit are owned and controlled by the United States Government, and form part of the San Gabriel Forest Reserve. Messrs. Staats and Holmes, Professors Campbell and Hale of your Committee, and Mr. T. P. Lukens visited Mount Wilson while I was there, and during their stay a number of questions which would arise in practical form if it were decided to place an observatory on Mount Wilson were dis-

cussed. From these discussions it was learned that the present owners would readily transfer a part of the mountain top, including the summit proper, for the use of an observatory; that they would assist in maintaining and fostering the growth of trees and other vegetation; that they would in every way safeguard the purity and permanency of the water supply; and, finally, that they would use their influence to make the summit more accessible by having an electric railway constructed to the mountain top.

The Pasadena trail, owned by the Toll Company, extends from the summit of Mount Wilson to the foot of Eaton canyon, a distance of 9 miles. It is constructed on a grade which is said to be 10 per cent. in most places, but nowhere exceeding this. The trail was located with the idea that it should eventually be converted into a wagon road. This could be done without much difficulty in most places. Portions of it, however, would require rock excavations and the construction of retaining walls, and some bridges might be necessary to avoid some of the present abrupt turns.

The fact that all the land excepting the portions enumerated above is held and controlled by the United States as part of a Forest Reserve is a very important consideration. It insures a constant patrol of the adjacent regions by the forest rangers, a safeguarding against forest fires, which are at times very destructive in semi-arid regions; a regeneration of the forest trees; a saving of the deep accumulation of humus, and thereby a conservation of the water—all matters of vital importance to the well being of an observatory whose work is to be largely solar research.

The San Bernardino mountains have been mentioned as lying to the east of the San Gabriel range, and as culminating in the San Bernardino and San Gorgonio peaks, about 40 miles southeast of Cajon canyon, which separates the two ranges. I did not visit these mountains, but from the time of my arrival in southern California until I came away I made many inquiries and heard much concerning them, particularly from those who have lived in them and made a study of their characteristics. From all the information that I could gather from the most reliable sources it did not seem probable that I should find in these mountains any location which would have conditions favorable to a high grade of astronomical work—conditions comparable to those that exist in the San Gabriel mountains. In reference to these matters I have particularly consulted with Mr. Lukens. In his opinion there is only one region in the San Bernardino mountains that would be at all worthy of consideration, and that would

not have advantages at all comparable with those of Mount Wilson. This place is Rogers cliff, on the Arrowhead grade, near Squirrel Inn Resort. It is said to be a delightful vicinity and one in which lumbering operations have been conducted intelligently. But the land is held in private ownership, and there is no assurance of the timber being renewed, as on the Government reserves. Sheep, which have devastated large areas in other sections, have been kept out of this one. The elevation is about 5,500 feet.

A large portion of the Mohave desert is immediately north of the San Bernardino mountains, and the Conchilla and Colorado deserts lie to the southeast. In the midst of the range, at an elevation of from 5,000 to 7,000 feet, there are valleys of considerable extent, the most noted of which are Bear valley and Little Bear valley. The former lies directly north of the highest peaks of the range and about 10 miles from their summits. The ridges to the north generally rise not more than a few hundred feet above the floor of the valley before they begin their rapid descent into the desert, which here reaches an elevation of about 4,000 feet. In winter snow often covers the ground to a depth of several feet, but when it melts a desert wind may carry away the moisture, leaving very little water. The summer days are warm, or even hot, and the nights are always cold. Even on summer nights it often freezes.

Little Bear valley lies farther west and has an elevation of about 5,500 feet. It has no considerable ridges between it and the Mohave desert, which lies 2,000 feet below it and only a few miles away. Mr. Lukens states that he camped for two weeks in this valley in June, 1899. At that time the temperature rose into the eighties during the day and fell below freezing every night. From temperature considerations alone, it is evident that these valleys would not be desirable locations for an observatory.

The San Bernardino Forest Reserve covers the San Bernardino mountain region, but the Government does not now own or control the best portions of the forested areas. Before the region was set apart as a reserve, private and corporate owners had secured control of practically all the valuable timber in accessible locations. Lumbering operations have been extensive. Logs are sawed at the mills and the lumber is hauled down to the plains. Roads have been necessary for these operations. The steep slopes of the mountains from the plains on the south to the crest of the ridge where the timber and mills are situated made it very expensive to secure easy grades for the various roads that connect the two sections. These are all

toll roads, owned, at least in part, by the same corporations that control the bulk of the valuable timber of the reserve. By making the rates of toll on timber over these roads practically prohibitory, these corporations have made it impossible for any one to conduct lumbering operations except under their auspices or by their permission. In this connection the following statement, taken from a report by Mr. John B. Leiberg* on the San Bernardino Forest Reserve, is of interest:

"There is, however, a point to which I desire to call attention. This relates to the means of communication with the forest areas from the plains. On the Mohave side of the uplift a number of roads and trails lead into the reserve. These roads are free and communication over them with the interior of the reserve is easy. On the western slope the only wagon roads leading into the forested tracts are toll roads owned by private corporations. The section of road from Highland Mill to Bear valley, along the crest of the ridge, is likewise a toll road. These corporations charge persons traveling on Government service toll, or not, according to their pleasure. The Arrowhead Company customarily refunds the toll or gives a pass upon proper application; but not so with the Highland Company, controlling the City Creek Road. Considering the fact that the different corporations own the bulk of the commercially valuable timber which grows in the reserve, and that the Government, in patrolling the reserve in general, protects this timber as well, it would be but a matter of equity that officers of the Government should at all times be entitled to the free passage of these roads."

As a result of the lumbering operations, the roads that lead into these forest regions are much traveled, and in summer they become very dusty. Some of them have much clayey road material, which grinds into very fine powdery dust. Parts of the City Creek trail are of this character. It has been said that "in all the State of California there is not another road so dusty."

I am not in possession of much information concerning the winds in the San Bernardino mountains. It is always stated, however, by those whom I have heard express any opinion in the matter, that winds prevail there to a greater extent than in the San Gabriel range. From what has already been said of the winds in the vicinity of the Cajon pass, and from the proximity to the Mohave desert, we should reasonably expect more wind, at least in the western and more exposed parts of the San Bernardino range, and, along with it, much desert dust high in air.

*U. S. Geol. Survey, Twentieth Ann. Rept. 1898–99, part V, Forest Reserves, p. 454.

To sum up: In the San Bernardino mountains there would in all probability be more wind and more dust, and certainly much greater daily range of temperature and less contact with civilization, than in the San Gabriel mountains. On the other hand, they are much less steep, and already have graded wagon roads to the crests of their ridges.

San Jacinto peak rises 10,805 feet above the sea. On its eastern side it is said to be the highest vertical wall in the United States. From the summit one looks over the San Bernardino plains to the west and over the Colorado desert to the east.

Strawberry valley lies southwest of the peak, half-way down, 5 miles away in a straight line, but 12 miles by the trails that one must take. It is well covered with pines and oaks, and is in the midst of many thousand acres of similar timber. It has numerous springs of pure water and several streams that never fail. The valley is wide and open, and is said to have numerous small elevations within its limits which rise some hundreds of feet above its floor. Tahquitz valley (7,500 feet), Lily rock (7,973 feet), and Tahquitz peak (8,826 feet) overlook Strawberry valley from the east. From what we know of mountain temperatures, we should expect these higher elevations to cause large daily variations of temperature in the valley below them. In themselves these higher levels are too elevated and too inaccessible to be desirable sites for a permanent astronomical station.

Many earthquakes are felt in the San Jacinto mountains. Tahquitz peak is especially noted for them. This circumstance alone counts heavily against the region for astronomical purposes.

I did not make a personal inspection of the mountains in the vicinity of Santa Barbara. They were not considered when the work in southern California was outlined. Later the question arose whether a really fine site for an observatory could be found there, and I then made inquiries concerning them. I have particularly consulted Mr. Lukens, and the salient facts respecting these mountains are given in the following paragraphs:

The Santa Ynez mountain is a ridge several miles in length, about 5,000 feet in altitude, lying parallel to the coast and rising abruptly back of the city of Santa Barbara. Its summit is accessible by a trail, and its air line distance from the sea is only 5 miles. Water is not found anywhere near the summit. There is but little timber on the mountain, and only a fair covering of chaparral—not nearly

so much as there is on Mount Lowe. For many years this has been an extensive sheep range.

The Santa Ynez river lies back of Santa Ynez mountain and is about 2,000 feet below the summit. It is the nearest source of water supply. It is said that the water supply of the city of Santa Barbara will eventually be taken from this river, as soon as the tunnel through the mountain is completed.

Pine mountain is farther inland. It rises to an elevation of about 6,000 feet, and is well covered with timber. The desert lies immediately north of it, and is only about 2,500 feet below the summit. Here there would be no protection from the desert dust and wind. Moreover, it is very inaccessible, as is the case with most of the mountains inland from Santa Barbara. At present there are no roads and no trails over which instruments could be taken.

The mountains north of Santa Barbara are more promising than those to the southeast, and yet they do not have conditions as favorable as one would desire. It is pretty nearly an arid country about Santa Barbara. Toward the southeast it is even more barren than in the immediate vicinity of the city. This is a conglomerate region, in which water is exceedingly scarce.

In the beginning it was not intended that an examination of any portion of Arizona should be made. On his return to Chicago from Pasadena, Professor Hale was much impressed by the transparency of the air over the desert. This led him to urge a brief inspection of northern Arizona. For this inspection I made a trip to Flagstaff and remained a week at the Lowell Observatory—from the evening of July 7 to the morning of July 15.

In response to an inquiry by Professor Campbell, Director Lowell very kindly placed the 24-inch telescope at my disposal, and besides did everything possible to facilitate the work.

In the late afternoon and early evening, Mr. Lowell was using the 24-inch telescope on Venus and Mars. Mr. Slipher was away and the spectroscopic work was discontinued during his absence. This circumstance gave me a large measure of time with the telescope without interrupting the regular routine of the observatory.

I was at Flagstaff at an unfavorable season. For a month or two in the summer there are almost daily thunder showers in this elevated region. Some of these are heavy storms, as was that on the day I left; but more often they are quite local and of short duration. During the week I was there clouds usually formed in the forenoon, sometimes early in the morning, and apparently first in

the vicinity of the higher mountains, such as the San Francisco peaks, etc. In general the clouds covered only a part of the sky, and their drift was slow. Observations were frequently interrupted by them. During the middle of the day the cloudiness increased, and thunder storms followed. Late in the afternoon it would clear and the nights were generally cloudless.

As a result of these unsettled conditions it was not expected that the seeing would be at its best, and during my stay Mr. Lowell did not regard it so. To me it appeared only fair. At no time did I rate the seeing above 3 on my scale. However, a comparison of his standards of excellence and mine was interesting and useful.

I took with me to Flagstaff the helioscope loaned to the expedition by the Yerkes Observatory. Mr. Lowell very kindly had it fitted to the 24-inch telescope, thereby making direct observations of the sun possible. A number of very large sun spots were visible, and much detail in them could be seen at times, but the moments of steady images were of short duration. For the most part heat waves followed each other rapidly, causing much movement of the image and some blurring.

When observations of the sun were made with the large telescope the aperture was reduced by diaphragms placed in front of the objective to 9, 12, or 18 inches.

In addition to the direct observations, the sun's image was occasionally projected upon a sheet of paper, usually to a scale of about 3 feet to the sun's diameter. Most of the observations of the sun were made during the forenoon. The earliest ones were made about 7 a. m., and they were usually continued at intervals until about 1 p. m. On one of the days the sun was also observed about 5 p. m.

Observations of stars were made at night, after Mr. Lowell had completed his work on Mars, usually until about midnight. On the whole, the conditions during this portion of the night were much better than those during the day. Double stars having equal components and distances greater than 0."25 were easily separated on most of the nights, and the measurement of much closer pairs would not have been difficult. Nevertheless, the seeing was not at any time of the highest quality. The diffraction pattern of a star's image was always more or less broken and in motion.

During my stay at the Lowell Observatory I detected two double stars, which appear to be new, and verified the duplicity of a third which I had suspected with the 16-inch telescope of the Lowe Observatory. These pairs are as follows:

D. M. +51°	1808	12h 59m 08s.3	+51° 46′.8	Close pair, companion of ε1718.
+48	2108	13 13 28.8	+48 32.6	8.5 and 11.0, Est. 1″.
+48	2243	14 44 15.6	+48 50.9	A close pair.

While we were at the Lowell Observatory the wind during the day was usually light and always from the southwest. Late in the afternoon it would die down and remain quiet for some hours. This is said to be the normal summer condition. Mr. Lowell states that at Flagstaff the best seeing of the day is usually at this turn of the tide—in the afternoon when the wind is dying down and at the corresponding period in the morning before it rises. This accords with the experience of Mr. A. E. Douglass, who has very kindly placed at my disposal the following statement concerning the seeing at Flagstaff from his forthcoming article on the "Selection of observatory sites":

"The seeing at Flagstaff, Arizona, is dependent on the general and local topographic features and the prevailing winds. The town is situated on the southwest margin of the great Rocky Mountain plateau. On account of the great size, elevation, and barrenness of that plateau, insolation and radiation are stronger than on the other desert regions touching it on the southwest. There is, therefore, an indraft toward this plateau from the southwest in the daytime, stronger in the afternoon owing to the insolation, and an overflow from the desert at night as the cold air settles down upon it, owing to radiation, reaching a maximum in the late night. The normal winds at Flagstaff, therefore, are southwest in the day and northeast at night. In summer, when the days are longer, the day condition prevails, and the prevailing clear-weather wind is southwest (changed to southeast when bringing the summer rains); in winter, with its length of night, the night condition dominates, and the prevailing clear-weather wind is northeast (changed to southwest in a storm area). It is the sunset and sunrise lulls between these day and night winds that give us the periods of best seeing.

"This explanation of the good seeing at sunset and sunrise has been verified in many ways. During the observations of Eros at the Lowell Observatory, when it was necessary to observe at least twice during the night, once early and once late, the late seeing on clear nights was always worse than the early, and it was usually accompanied by a current of air from the northeast. This is the overflow from the desert, as already explained. This overflow, coming on rather suddenly late in the night, is also the explanation of the sudden rise in temperature in the valley of Flagstaff in the summer, at three or four o'clock in the morning, frequently found on the thermograph records. Previous to this hour the quiet air in the valley of Flagstaff has been growing steadily colder; but when the overflow occurs in the form of a fresh northeast wind it

carries the settled cold air out of the valley and raises the temperature.

"The southwest margin of this desert plateau is a ridge called in part the Mogollon mesa. It follows the line of a great fault through Arizona from northwest to southeast. This ridge acts like a dam to the rising warm air by day and the down flowing cold air at night. Flagstaff is at one of the low points on this dam, and therefore gets a particularly pronounced effect from this interchange of air between the high and low desert regions. Its winds are probably stronger than those anywhere else along the ridge and they begin earlier and last later.

"Now as the cause of unsteady seeing is the multitude of irregularities of density in the air, produced by contact with heated or chilled surfaces, and as, also, these irregularities are brought over the telescope by aid of the wind, we may generalize and say that the badness of seeing increases with wind and the opportunity that wind has of contact with heated or chilled surfaces. We have seen that Flagstaff is particularly subject to strong winds as compared with other parts of Arizona; we have now to study the opportunity that wind has of contact with heated or chilled surfaces. It is perfectly evident that hills and mountains, reaching up into the great mass of moving air, present the best opportunity for such contact. On examination we find that the entire horizon of Flagstaff from north-northwest to east-northeast is occupied by a mountain range, rising from 2,000 to more than 5,000 feet above the town, and distant from it between 4 and 12 miles. The winds that pass these peaks are not only diverted in a vertical direction, which causes change in their density, but also, by contact with their warmed or chilled surfaces, suffer small irregularities in density throughout their mass. Hence, whenever the wind blows from a northerly direction the seeing becomes poor in proportion to the velocity of the wind and its directness of passage over the mountains. Therefore in winter, when the northeast wind predominates, the seeing is generally poor.

"This explanation of the bad seeing in winter has been frequently verified by testing on the same night the atmosphere at Flagstaff and at points as little as 12 miles east, out of the lee of the mountains. Even when the seeing at Flagstaff was frightfully bad, Eros showing a diameter of 13 seconds, the seeing away from the mountains was perfectly fine and steady.

"Thus the times of best seeing at Flagstaff are at sunset and sunrise in the non-winter months, and the poor seeing at other times results from the topographic features and the prevailing winds.

"It should be remembered that this criticism of Flagstaff seeing is derived from a minute comparison of it with other parts of the southwest arid regions. Compared with eastern localities I regard its qualities as very superior."

I took with me to Flagstaff one of the Draper thermographs belonging to the equipment of this expedition, and during the week

I exposed it in the thermograph shelter of the Lowell Observatory. The range of temperature shown by the record was 29°, from 59° to 88°. The greatest variation in any one day was 24°.

The record during the day was generally full of small variations, in addition to the larger changes. This was probably due to the passing of more or less extended clouds, giving intervals of sunshine and of shade, but no direct test of this hypothesis was made.

The temperature at night was more uniform than by day, but the night curve did not always have the smoothness that one likes to see. However, on some of the nights there would be an interval of five or six hours during which the change of temperature would amount to only one or two degrees.

Mr. Lowell very kindly placed at my disposal the thermographic record of the observatory. The time available did not permit a detailed study of the entire record, but I examined with some care the sheets for the year 1902.

Some of the general characteristics of the record were at once evident. The fluctuations during the daytime were often large, and superposed upon these were frequent minor variations. In passing from day to night temperatures there was, as is always the case, a pretty rapid change during the early hours of the night. This period usually lasted about three hours—to take the year through, from about 6 p. m. to 9 p. m., but earlier in winter and later in summer. This, however, was not the rule. After the early evening hours the temperature gradient would generally become less steep, or even nearly horizontal, save for minor fluctuations.

A feature worthy of special notice was prominent on many of the sheets. Often in the latter part of the night, generally after 3 a. m. and often as late as 6 a. m., the temperature would suddenly fall from 5° to 15°, and then after a short time rise again, partially regaining its former height. As an example, on the sheet I obtained there is shown on the morning of July 14 a sudden drop of about 7° shortly after 3 o'clock a. m., and half an hour later a rise, nearly as sudden, of about 4°. I have been told that these sudden changes of temperature are here associated invariably with very bad seeing, which is entirely in accord with what might be expected under such circumstances.

Respectfully submitted.

W. J. HUSSEY.

MOUNT HAMILTON, CAL.,
August, 1903.

APPENDIX B TO REPORT OF COMMITTEE ON OBSERVATORIES

LETTERS RELATING TO SOUTHERN AND SOLAR OBSERVATORIES.

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I. CORRESPONDENCE RELATING TO PROPOSED SOUTHERN OBSERVATORY.

Extracts from Confidential Statement.

The following extracts from a confidential statement, prepared in June, 1903, are prefixed to the letters relative to the proposed Southern Observatory, in order to render more intelligible the comments upon the statement contained in the letters:

"The Advisory Committee on Astronomy for last year of the Carnegie Institution (Messrs. Pickering, Langley, Newcomb, Hale, and Boss) recommended the establishment of two observatories. One of these was proposed for the southern hemisphere, to assist in reducing the disparity which now exists relative to the observation of astronomical objects in the two hemispheres. The other is a proposed solar or astrophysical observatory, to be established in the best practicable atmospheric conditions and with a powerful equipment.

"The Institution is not in any way committed to either of these enterprises at present. * * * Accordingly a special commission of three astronomers (Messrs. Campbell, Hale, and Boss) was appointed to investigate and report more fully in regard to these recommendations. * * *

"Among the special observations that are regarded as important are the following:

(1) "A fundamental determination of star positions, with extension of these observations by secondary methods to include every star brighter than the seventh magnitude between —20° and the south pole. * * *

(2) "Observations for stellar parallax have been secured at the Cape Observatory. Against this one observatory we find several observatories in the northern hemisphere actively engaged in deter-

minations of stellar parallax. * * * Therefore, determinations of these has been considered an important feature for the work of the proposed observatory.

(3) * * * "It is considered highly desirable that another very large reflector should be provided for this use [determination of radial motion] in the southern hemisphere. Were such an instrument provided it could also be employed in other spectroscopic researches requiring powerful optical means and also in the photography of nebulæ. * * *

(4) "The extension from —32° to the south pole of zone observations upon the general plan adopted by the *Astronomische Gesellschaft*. The Cordoba Observatory has secured the necessary observations for the zone —22° to —32°. Only one fourth of the sky remains to be covered. * * *

(5) "It is thought desirable that extensive observations for variations of latitude should be made in the southern hemisphere somewhat upon the plan now adopted for the northern hemisphere in the international service. * * *

(6) "During recent years, and especially at the Lick Observatory, very thorough surveys of the stars in the northern sky have been carried out for discovery and measurement of close double stars. It is considered desirable that similar investigations should be carried out with at least one large telescope for that part of the southern sky which is beyond the reach of northern observatories. * * *

(7) "Eventually certain astrophysical researches of precision, requiring great optical power, will be required in the southern hemisphere to complete the evidence relative to certain classes of objects which have been, or are about to be, investigated in the northern hemisphere. * * *

"In view of these considerations, it would be of great service if the consensus of experienced astronomical opinion could be obtained in regard to the relative urgency of observations which ought to be undertaken now in the southern hemisphere and which are inadequately provided for at existing institutions there, whether such observations have been alluded to in this statement or not. * * *

"It would also be useful to learn whether any similar plans are known to be under consideration by others; or whether there is any prospect of such. * * *

"Letters have been addressed to astrophysicists in relation to the proposed astrophysical observatory for which a site in southern California is contemplated. The object of this observatory would be to investigate the physics of the sun, and, especially, the amount and variation of solar radiation, with apparatus more powerful than, and in conditions of atmosphere superior to, any which have been hitherto available for the purpose. Your suggestions and advice upon this head would also be very acceptable." * * *

LETTERS FROM CORRESPONDENTS.

[*From Director H. H. Turner, of the University Observatory, Oxford, President of the Royal Astronomical Society.*]

OXFORD, *July 17, 1903.*

The Statement deals chiefly with the project for a Southern Observatory, and this reply will be confined to that project. Before considering the points of detail raised in the statement I should like to express an emphatic opinion that there is now, as there has always been hitherto, a great need for assistance of astronomical observation in the southern hemisphere. Various spasmodic attempts have been made in the past to bring our knowledge of the southern hemisphere into better relation with that of the northern. Some of these have been successful, others have failed: and it will help us in considering the matter if we glance at a few instances of both kinds. As unsuccessful attempts I will quote—

1. The project initiated by a Committee of the Royal Society half a century ago (see Mon. Not., R. A. S., XIV, 129) for a large reflecting telescope in the southern hemisphere. The Melbourne telescope was provided, but very little has been done with it.

2. The project for a Southern Survey (see Mon. Not., XXIII, p. 147; XXV, p. 118; XXVII, p. 132; XXIX, p. 161) which seemed to be making some progress, but eventually died out.

3. The southern share of the Astrographic Chart, which is lamentably behindhand at present; three of the southern observatories which originally undertook a share never made a start, and two of those which replaced them are in great need of assistance. At others the work is going on very slowly, except perhaps at the Cape Observatory.

For comparison with these we may cite various successful enterprises, and it will be seen that they are either of a purely expeditionary character or that at least the workers remained in touch with the northern hemisphere.

4. The expeditions of Halley and Sir John Herschel to the southern hemisphere, which successfully advanced our knowledge of that hemisphere at the epochs of the expeditions.

5. The expedition, in recent times, of Mr. McClean, who extended his spectroscopic survey of the bright stars to the whole sky and incidentally discovered oxygen in β Crucis.

6. The semi-permanent expedition from Harvard to Arequipa, the material collected at Arequipa being returned to Harvard for examination and discussion. Among other discoveries, this has given us several "new stars."

7. The work done by the late E. J. Stone in forming the Cape Catalogue, 1880, which was largely expeditionary in character. Mr. Stone remained at the Cape only 9 years, and went out definitely to do this single piece of work.

8. To some extent the same may be said of the work done by Dr. B. A. Gould at Cordoba.

9. In considering the eminently successful work done by Sir David Gill at the Cape Observatory, it may be remarked that he has paid frequent visits to Europe and kept himself in close touch with the northern hemisphere throughout. In the case of one large piece of work, the Cape Photographic Durchmusterung, the measurement of the plates was conducted by Professor Kapteyn in Europe.

No conscious selection of the above nine instances has been made, though it is quite possible that I have been unconsciously influenced by the opinion already indicated: that much, if not all, of the successful work done in the southern hemisphere has been accomplished either by observers who have made a purely temporary expedition from the northern hemisphere, or by men who have kept closely in touch with the North, or quite recently by the examination, in Europe or America, of material collected in the southern hemisphere.

And if this is true, it is, after all, only natural. The permanent observatories in the South have to contend with the gravest disadvantages. The governments at their backs are comparatively poor; there are heavy claims upon them for other work, such as meteorology (as at Sydney), or telegraphy (as at Adelaide), and they are far removed from the advantages of the older civilizations in respect of intercourse with men of science.

The lessons inculcated with regard to future projects are tolerably obvious, and I cordially welcome the general proposal to establish a Southern Observatory of the *expeditionary kind* indicated in the statement. I proceed to consider the points of detail dealt with in the statement.

The Observations Considered Important.

1.* *Urgently Needed.*—Personally I should like to see the photographic method used. I have elsewhere indicated (Mon. Not.

* The figures refer to the numbered paragraphs in the Confidential Statement, pp. 106-7.

R. A. S. LVII, p. 349) the lines on which I feel sure a simple and efficient photographic transit circle could be constructed. The heavy work of the Astrographic Chart has compelled me to defer any trial of this method, but having nearly completed our measures we are now on the point of trying it, and possibly information as to its success may be forthcoming at an early date. The "magnitude equation" is so troublesome for eye observations that I feel sure we ought not to delay the introduction of photography into meridian work any longer, and I feel confident of the success of the method suggested. No new departure is contemplated beyond mounting a mirror on pivots instead of providing a telescope, and this should offer no difficulty—rather the contrary. But I am well aware that no instrument is complete so long as it is only on paper.

2, 3, 4, 5, and 7. I thoroughly agree with what is said and have no special remarks to make.

6. *Double Stars.*—The work of Mr. R. T. A. Innes at the Cape Observatory should be remembered; but Mr. Innes has recently been appointed director of a new observatory where this work may not be possible, since the first call on his energies will be from the *meteorological side*. Compare what has already been said about the non-astronomical calls on the attention of directors of southern observatories.

Other Observations not Mentioned in the "Statement."

8. *Planetary observations of position* from the southern hemisphere are in some ways as desirable as stellar. Sir David Gill has already started observations which will supply several of these wants; but I should like to draw attention to the fact that a series of observations of the moon which would give good places *throughout the lunation* during at least one year would certainly improve our knowledge of the parallactic inequality; and for completeness we should have series obtained at both northern and southern stations, unless a series could be obtained from near the equator, as Madras or Kodaikanal. The chief requisite is a *fine climate*, such that the moon could be caught very near new moon and pretty continuously in at least one or two months.

There are various methods which might be used; to give one example, photographs might be taken with a telescope clamped for the night (*a*) of the moon and (*b*) of stars preceding or following by a known interval. These photographs could be measured and discussed elsewhere if necessary.

9. At some time a number of longitudes of stations in the southern hemisphere must be determined with modern precision.

While not immediately pressing, this work might become pressing at any moment; and it could scarcely fail to be of ultimate service if a new station were fixed relatively to (say) the Cape Observatory with accuracy.

Similar Plans under Consideration.

I know of no similar plans. On the other hand I have had special reasons for fearing that the total available energy of southern observatories will for the next twenty years be almost completely absorbed by the work of the Astrographic Chart. On this point I have already addressed a letter to the Chairman of the Committee, and perhaps I need not here repeat what I said.

Location.

I have made inquiries about

(*a*) *South America*, which would in many ways be suitable, as differing in longitude from existing observatories. For stations of considerable southern latitude the replies have not been favorable.

(*b*) *South Africa.*—I hear very good accounts from Sir David Gill and Mr. Innes of the site selected by the latter for the new observatory of the Transvaal.

(*c*) *Australia.*—Mr. Russell gives good accounts of sites near Sydney. Recently I have heard sites near Hobart Town, in Tasmania, much commended. The climate is said to be beautiful and land so cheap that a large tract could be secured.

General Plan of the Work Temporary.

I have already commented upon this point at the beginning of this letter. To keep in view the "accomplishment of a few specific works, the conclusion of which could be foreseen within a limited term of years," seems to me to be exactly the way to set about the advancement of the astronomy of the southern hemisphere, if we may trust previous experience; and it seems to me further quite probable that there are several men, perhaps many men, of proved capacity and experience, who would find an expedition to the southern hemisphere desirable and delightful if it could be undertaken without financial loss and for the completion of a piece of work already carried out in the northern hemisphere. I understand that this kind

of expedition is in the minds of the Committee, and it seems to me to be most attractive, and to promise an economical solution of the problem of dealing adequately with the southern hemisphere on lines suggested by a study of past history.

[*From Director M. Loewy, of the Paris Observatory, President of the Congress for the Carte du Ciel.*]

[Translation.]

PARIS, *July 20, 1903.*

The establishment of two observatories in the southern hemisphere would be, without any doubt, of high utility for astronomy.

The program prepared by the Committee in regard to the work to be accomplished there, seems to me, in its broad lines, most judicious. You have expressed in your communication two ideas which are in perfect harmony with my own personal view: First, that large scientific enterprises often stand in intimate relation with each other, which brings it to pass that the success of the one instigates progress in the others; Second, that it is desirable to have at our disposal a large number of carefully determined proper motions in order to undertake with profit some of the noblest problems which today are occupying the minds of the most eminent astronomers. It is only in this way that one can acquire more precise conceptions of the nature of the trajectory of our solar system and of the general structure of the universe. I take this point of view in communicating to you the reflections suggested by a perusal of the program of the committee.

First of all, I should like to suggest a slight modification of your plan of work concerning the execution of the meridian observations between $-32°$ and $-90°$ of declination, in order to give to this research a more general utility and to render it valuable in the construction of the Astrographic Chart. In order that you may better appreciate the drift of the remarks which I submit to you, allow me to present some explanations concerning the details connected with this work. The recent researches which I have published on the subject of the precision with which one can take the astronomical coordinates of the stars from a photograph show that by exercising the necessary precaution one can attain such precision that the total probable error would reach at the maximum $\pm 0.''2$ even for the faintest images. This determination is not in the given instance an *optimistic* estimate; it represents the reality. It is well to add that

a *great part* of the probable error above indicated springs from the inexactitude with which the positions of the comparison stars, chosen for the calculation of the constants of the plates, are affected.

From this one may conclude that if the photographic observations should be repeated twenty years from now under the same conditions of precision we should be able to determine with accuracy the proper motions of about 100,000 stars, as I have indicated on a former occasion.

Besides, in that which concerns the construction of the Astrographic Chart, which includes the exact positions of more than 3,000,000 stars, twelve observatories out of the eighteen in this line of work have not only completed their observations, but have also concluded the task of measuring the rectilinear coordinates that correspond to them.

One of the essential requirements under which the degree of approximation mentioned above could be equaled, and even surpassed, lies in the determination of the plate-constants with all possible accuracy. In investigating this question, the majority of the observatories associated in this work have decided that, if the positions of the comparison stars should be taken from the catalogue of the Astronomische Gesellschaft, their accuracy would be insufficient and would not correspond with that which obtains in the measurement of the rectilinear coordinates. It is for this reason that out of the twelve observatories above mentioned seven proceeded to the direct determination of their comparison stars with the help of precise meridian observations.

It seems to me, then, that the general catalogue which you are proposing to observe between —32° and —90° would have a much higher value if, aside from all other applications, it could supply the comparison stars necessary for the reduction of the plates of the Astrographic Chart included between —32° and the south pole, entrusted to the observatories at the Cape, at Melbourne, Sydney, and Perth (western Australia).

Influenced by these considerations, Sir David Gill has already determined the positions of 8,556 comparison stars of his zone by very careful meridian observation—about twelve for a plate and three meridian observations at least for each star.

To fulfil the requirements for the chart of the heavens relative to this region, it would be necessary to obtain the accurate positions of about 30,000 stars, which would give about twelve comparison stars for each plate of four square degrees.

Instead of observing in an expeditious way, according to the methods recommended by the Astronomische Gesellschaft, 60,000 stars, requiring, according to your estimate, about 150,000 observations, I would prefer that the positions of 30,000 only should be determined with greater accuracy. This would possess the double advantage of giving, on the one hand, a very precise basis for the photographic catalogue of this part of the sky, and, on the other hand, of providing a solid foundation for determination of the proper motion of the stars. It is very desirable that we should have at our disposal, at an early day, the results for this element, which is of such fundamental importance in modern astronomy. With this object in view, to make observations with twice the degree of accuracy is to reduce by one half the interval of time necessary to render sensible the proper motions of the stars.

It would also be desirable, in order to place the catalogue on a sure and homogeneous foundation, to combine it with the 6,000 stars above the seventh magnitude which you propose to determine with the most rigorous precision. A catalogue constructed under these conditions would certainly serve in the future as the starting point for all researches of high precision concerning the study of proper motions between —32° and —90°.

Before leaving this subject I will present another example to illustrate its great value. In addition to the plates of short exposure intended for the construction of the Astrographic Chart, comprising all stars down to the eleventh magnitude, it is also intended at the same time to secure plates of long exposure, guarded by the same precautions and including stellar images down to the fourteenth magnitude. Then, if necessary, the plates of this second series could be measured and reduced with the same degree of precision as the first. Since a great many of the plates contain the images of 1,000 to 12,000 stars, one can readily see that, after photographing for a second time one of these regions so rich in stars, the comparison would reveal a very large number of proper motions for all grades of magnitude down to the fourteenth, a feature of this plan which might prove very instructive from the cosmogonic point of view.

In your program you point out the necessity of determining the parallax of stars; the value of such work is incontestable.

In my opinion the surest method and the one least subject to systematic error is the photographic method properly applied. It would therefore be very judicious to provide for this purpose a

photographic instrument similar to that used for the Astrographic Chart. This instrument could also render valuable service in other lines of work.

Among the eighteen observatories that were associated in the work of the Astrographic Chart there is one that has not held to its engagement. This is the observatory at Santiago, Chile. It is in order to fill its place. You could furnish invaluable assistance to this vast scientific enterprise should you also devote this instrument to the work of the Astrographic Chart.

In a favorable climate an experienced observer, aided by two assistants, could accomplish it in about eight years and could secure all the plates for this zone—1,260 plates for the catalogue and 1,260 for the chart. This period of time should suffice, now that all the methods are so well established.

In providing the catalogue of comparison stars, or by undertaking the vacant zone, or, better still, by accomplishing both tasks at the same time, America would find her opportunity to occupy a most honorable place in this vast international enterprise, whose success, which is now assured, will be without the least doubt the occasion of numerous and noteworthy discoveries.

Finally, it seems to me proper to mention a very useful and inexpensive work which could be undertaken by one or the other of the two proposed observatories, and which up to the present time has been wholly neglected in the southern hemisphere—the systematic observation of meteors. An experienced astronomer, with the aid of one assistant, in three or four years would be able to produce important results in this line. It is in respect to the discovery of stationary radiants, the activity of which lasts for weeks and even months, that he would supply most valuable material for the study of the causes and conditions which produce this curious phenomenon. On the other hand, one could also reasonably hope to run across some notable swarm of meteors in connection with the periodic comets, examples of which, still so rare (four in all) have been observed only in the northern hemisphere. This would be a particularly desirable contribution, which might render possible a more searching investigation of the close relation existing between comets and meteors; also comparison of the frequency with which meteors appear in different seasons of the year cannot fail to bring enlightenment on the origin and velocities of meteors.

In that which concerns the location of the astronomical station the latitude should certainly not be north of —30°, as you have

indicated. It would be advisable at the same time to seek for a climate as free as possible from dust and cloudiness. Perhaps a desirable location could be found in the Argentine Republic, preferably a little south of Cordoba. The climate of that city seems to have lost in quality during the last few years; the winds filled with dust have become more frequent.

[*From Director H. Bruns, of the Royal Observatory, Leipsic.*]

[Translation.]

LEIPSIC, *July 23, 1903.*

In answering the questions proposed by you, permit me first to formulate the following propositions: Astronomy, now and in the future, is confronted with the task of attaining in reference to the universe of stars what has already been accomplished for the solar system, namely, a sure knowledge of the space arrangements and of the motions of the bodies therein contained. Furthermore, the duty is imposed upon each generation of astronomers to contribute toward the attainment of that end whatever is possible with existing means.

If, now, one reviews what has been already accomplished in this field, it will be seen that the weak point is not the extent and quality of available observations, but the unequal distribution of them upon the two halves south and north of the equator. From this inequality arises a sensible defect in all discussions which have for their subject, not single objects or groups of objects, but the heavens as a whole. Therefore, without any reservation whatever, I agree with you that an attempt should be made to lessen this defect, especially since an effective remedy through increase in the permanent observatories of the southern hemisphere is not otherwise to be expected at present.

As to that which concerns the individual propositions set forth by you, I am of opinion that the strengthening of the weight of exact star positions has precedence over all other things. Observations for parallax, velocity in the line of sight, etc., can be secured at any time, whenever the means are at hand, without fear that a marked disadvantage to the development of astronomy would result from such delay. On the other hand, there is one thing that cannot be retrieved through later observations, and that is the "epoch" of a catalogue.

Of the catalogue works mentioned in your communication, the extension of the zones of the Astronomische Gesellschaft to the

south pole is, in my eyes, the one most important for the future of astronomy. This assumes in advance, however, that the work designated by you as No. 1, for the brighter stars, should precede. Therefore, I entertain no doubt that proposition No. 1 should have the preference over all the other tasks.

[*From Director Küstner, of the Royal Observatory, Bonn.*]

[Translation.]

BONN, *July 27, 1903.*

I have read your letter of July 8 and the enclosed program of the Committee of the Carnegie Institution with the greatest interest, and I hasten to express to you my full and unqualified acquiescence in the propositions therein contained.

The situation is so clear and simple that, in my view, only one answer is possible to the question, "How can astronomy be promoted to the best advantage?" namely, through the establishment of an observatory equipped with the best instruments in the most favorable location in the southern hemisphere. The present neglect of the southern sky is felt in the most troublesome manner in all astronomical problems, and many series of observations that have been secured in the northern sky with great care and at great expense cannot be fully employed for the benefit of science because they pertain to only a part of the sphere. The most important conclusions can be reached only after these have been equally extended over the southern sky, and then only will the finest fruits of astronomical investigation begin to ripen.

I can but join in approval of the list of works, arranged in a preliminary way according to their importance, which is proposed for this Southern Observatory. This list might easily be further increased, but practically, at the outset, it may have to be curtailed.

I hold point 1 [meridian observation of stars down to the seventh magnitude] as the most important and—because closely related to it—point 4 [observation of all stars down to the ninth magnitude, southward from —32°]. The prompt continuation to the south pole of the great undertaking of the Astronomische Gesellschaft, which has already been extended down to —23° or —32° is an unconditional necessity. This last quarter of the sky must soon be worked out, if we are not to lose a considerable part of what has been accomplished in the three quarters already completed.

In the continuation of this plan of the Astronomische Gesellschaft

one will naturally profit by the experience which has been gained in the meantime. For example, the personal equation for magnitude should not be simply determined in an incidental way, but directly eliminated. Further, the zero-stars should be observed not merely at the beginning and end of the zones, but they should be regularly distributed throughout the zones, in such a manner that they should be observed at average intervals of about ten minutes.

For the next most important works I consider points 2 and 3 to have the preference [point 2, determination of stellar parallax; point 3, determination of the velocity of stars in the line of sight].

As to the question of choice of a suitable site, I am unfortunately unable to make any definite recommendation. The precision, and consequently the value, of astronomical observations is dependent in so large a measure upon the condition of the atmosphere, that, in the choice of site, regard for the best possible conditions of atmosphere must be the controlling factor, especially when an observing station is to be established for a few years only. If the question turns on the establishment of a permanent observatory, then, by all means, proximity to some center of civilization is also to be considered.

Furthermore, I can but agree in the most complete conviction with the proposition for a great astrophysical observatory especially for solar investigation. The only scientifically correct and at the same time practical way to attain the beginnings of knowledge as to the nature of the fixed stars is, in the first place, through most exact investigation of the star nearest to us, the sun, concerning whose constitution so many obscure problems prevail.

I hope that the Committee may be successful in making the large resources of the Carnegie Institution useful for astronomical investigation in the manner proposed.

[*From Director H. Seeliger, of the Royal Observatory, Munich, President of the Astronomische Gesellschaft.*]

[Translation.]

MUNICH, *August 3, 1903.*

The establishment of a new observatory in the southern hemisphere, with a large provision for instruments, must be regarded as a desideratum for astronomy. The opinions of astronomers will not differ in this respect, and it would be quite unnecessary to prove this in detail, especially since the Statement which you have been good

enough to send me handles the question completely to the point, and exhaustively. The problems that fall to such an observatory arise through the development of modern astronomy, and, indeed, in this Statement they are so fully and clearly enumerated that very little can be added. The main point is to establish the requisite balance between observations made upon the northern and the southern hemisphere, which, hitherto, the few southern observatories could not possibly maintain. Any arrangement, according to their importance, of the classes of observations that ought to be made will naturally depend upon the standpoint of the one who renders judgment, and therefore I can only say that I hold the order given in the Statement to be essentially sound. On the other hand, one can make good the claim that the astronomer has not only to collect the knowledge which will enable later generations to derive important results, but also that he should especially challenge those problems that will permit general conclusions to be drawn within a time not distant, even though in a fragmentary way only.

From this point of view I have for several years expressed the opinion that, though observations of fixed stars made to establish the *motions* of the heavenly bodies are important beyond doubt (though they will first bear fruit in the distant future), yet observations concerning the *present aspect* of the starry heavens should not be neglected. This aspect has an independent interest of its own, and from it a valuable result can be drawn at once without waiting for the cooperation of future generations. Accordingly, on account of their bearing upon my own investigations in regard to the space relations of the stellar system, I would like to designate as especially important the following problems for which the cooperation of the observatories in the southern hemisphere is absolutely necessary, since it is possible to make these discussions only upon the basis of observations distributed over the *entire* sky:

(1) Determination of parallaxes, which appears as (2) in the Statement.

(2) An investigation of the apparent distribution of the stars in the southern hemisphere, including those of the faintest magnitude that can be observed. The Statement excludes the consideration of new problems, to be sure; but here we are concerned, not with a new work, but with an old one which has not hitherto received sufficient attention, the object of which is to solve the new problems more conclusively and the execution of which would, in effect, establish the solutions upon a sounder basis. A similar work is now

being carried out for the northern hemisphere at the Munich Observatory, though in a very modest way, since the means at its disposal are very limited, and in this connection I would refer to my report in the Vierteljahrschrift of the Astronomische Gesellschaft.

(3) Very many—indeed, most—of the investigations in stellar astronomy depend upon the establishment of an exact photometric scale of magnitudes. For the northern sky, in this respect, the magnitudes of the stars down to about the eighth magnitude are fairly well established through the labors at Cambridge (Mass.) and Potsdam. In this connection is to be noted the demand for fixing by photometric methods the magnitudes of the fainter stars, through a satisfactory choice of objects, evenly distributed, down to the faintest which can be observed. This is still an object to be desired. For the brighter stars in the southern hemisphere an extensive series of observations is available. But undoubtedly this is not free from objection, and we are not in position to establish the distribution of the stars upon the southern sky in combination with that upon the northern hemisphere.

The Statement does not propose to take up photometric and photographic works for the present at least. But I think that the works designated under (2) and (3) are those that are now most pressing and, at the same time, those that would be most acceptable. Here, within a few years, if sufficient means exist, we may hope for results that will be of the greatest importance for all time. Of course, one would better observe with the same instrument first at a station in the northern hemisphere, and then from a station in the southern hemisphere, or one would have to choose a place in the neighborhood of the equator, and from that point survey the entire sky, which, of course, would not be entirely free from objection.

The choice of a place for an observatory I consider to be extremely important. When one reflects what limitations and disadvantages the climate in our latitudes impose upon all astronomical observations, one can only look with envy upon the astronomer who is permitted to live in a good climate. What a mass of provocations and waste of time is such an astronomer spared! I take this opportunity to call your attention to the Australian continent, or to Tasmania, whose wonderful and, at the same time, healthy climate I know from my own experience, though from no more than a residence of a few months.

If, in conclusion, I may present an arrangement of the works mentioned in the Statement, according to my opinion of their im-

portance in relation to the views I have expressed in the foregoing, I would arrange them thus:

1. Point (2) of the Statement (parallaxes).
2. Point (3) of the Statement (radial motions).
3. Point (1) of the Statement (meridian observations).
4. Point (4) of the Statement (zone observations).

Your further arrangement corresponds perfectly to my views.

At all events, a great and inestimable gain for astronomy would be realized if your views in reference to a new Southern Observatory should come to fulfillment, and it appears to me that in that event it is not so much a question *what* important works shall be assigned to the new observatory, but rather that there are in general important works which it has to accomplish, and that all in the Statement are such I am fully convinced.

[*From Sir David Gill, Astronomer Royal at the Cape of Good Hope.*]

CAPE OF GOOD HOPE, *12th August, 1903.*

You ask on behalf of your Committee my views on the subject of the most urgent needs of astronomy.

There cannot be the slightest doubt that from the highest standpoint what is most urgently required is an increase in the astronomical equipment of the southern as compared with the northern hemisphere, and this is equally true in the departments both of the older astronomy (astrometry) and astrophysics.

There are urgent needs in both of these departments. The relative urgency will vary, in the opinion of many, according as the individual's knowledge or sympathy lies with one department or the other.

Astrometry.

A, 1. In connection with the older astronomy, I entirely concur that the establishment of an additional meridian circle of the very first class in the southern hemisphere in an ideal observatory for fundamental observations is a first essential. Practically the Cape is the only observatory where really fundamental work is being undertaken, and some independent check or comparison is necessary if only to give assurance of the accuracy of results arrived at.

It may be remarked that most of the observatories of the northern hemisphere are defective in the form of covering or observatory for their transit circles and in the means of equalizing the internal with

the external temperature. Therefore, if any transit circle is to be sent to the southern hemisphere, and is to be used before or afterward in the northern hemisphere for the purpose of comparing results, it seould be provided, for both series of observations, with a modern steel observatory having double or triple walls, means of conveying the convection currents away from the observing opening, and of separating the whole observatory into two halves, so that the instrument may be used as nearly as possible in the open air.

There is a still more important condition which should also be fulfilled, viz., provision of means to avoid personal equations depending on magnitude or upon the velocity of the star's motion (*i. e.*, the star's declination). It seems to me that the only system of observing in R. A. which permits this possibility is the Repsold-Struve method, in which a wire is made to travel across the field at nearly the same velocity as the star. The eye piece travels with the wire, so that, if the mechanical conditions are properly realized, the observer, having bisected the star disc with the wire, should view the disc so bisected apparently as if at rest, and be able by simple means to correct any errors of this bisection which he may notice during transit and which may be due to errors of the clockwork, the driving screw, or the original pointing. The drumhead of the screw which causes the slide of the moving wire to travel is provided with contacts which, as the drum rotates, make electric contact with the chronograph circuit, and so record the instants when the pointing on the star would correspond with the particular readings of the micrometer head.

This method, so far as I am aware, is the only one not liable to personality depending on magnitude or declination, and this, although we have not yet absolute proof, we believe also to be free from personality in observations of the sun and moon.

The necessity for provision of reliable azimuth marks and of a clock not liable to diurnal variation of rate is too well known to require further reference.

A separate memorandum dealing with some details of the above-mentioned methods will be forwarded.

As to a site, I think it would be difficult to find in the southern hemisphere a better one for this purpose than the neighborhood of Bloemfontein. I venture to think that in connection with this plan one or two of the northern observatories should be provided with a better form of observatory or covering for the transit circle.

A, 2. There are two other problems of the older astronomy which cry out for solution, one of which is a comparatively small affair, the other a very big one, but both are urgent. I refer to the completion of the organization in the southern hemisphere for determining change of latitude (that is the smaller affair), and to the formation of a parallax Durchmusterung—*i. e.*, determining the parallaxes of *all* the stars to a certain order of magnitude—(that is the large one).

I take the simpler matter first. Mr. Chandler proposes a southern belt of observatories:

	Lat.	Long.
Sydney	−33° 51′	−151° 2′
Cape of Good Hope	−33 56	− 18 5
30 miles south of Santiago	−33 54	+ 70 7

It should involve very little trouble and comparatively small expense to establish the necessary organization at Sydney and the Cape, and I venture to think that the Carnegie Institution could not be better advised than to provide at once for the observatory near Santiago, equipped with two observers devoted wholly and solely to determination of the aberration constant and change of latitude. The instrument I would recommend for all the three observatories would be the photographic almucantar and the method used by Cookson (see Monthly Notices, R. A. S., LXI, p. 315). There should be observations of every group of stars in the early evening, the early morning, and near midnight, at all times when opportunities occur. In this way we ought to get an extremely accurate determination of all the latitude changes and a powerful determination also of the aberration constant.

A, 3. The parallax Durchmusterung is a very much greater undertaking, but it is of the supremest interest to science. I do not think it desirable to go beyond magnitude 9½ or perhaps 9; even then the taking and measuring of the plates is a very big business and involves a large organization.

A telescope of large aperture is not necessary, but considerable focal length is requisite to give the necessary precision of measurement. The highest optical perfection should be arrived at, probably a 4-glass objective of 8 or 10 inches aperture and 20 feet focus would be most suitable. I think it very undesirable to employ a cœlostat or any plan involving reflection from a plane mirror, as plane mirrors may be liable to flexure or deformation by temperature changes.

Kapteyn's method should be adopted, and all the photographs taken at different seasons on the same plate should be exposed at the same hour angle. Although this involves some sacrifice of parallax factor it eliminates many possible sources of systematic error. I would advocate an observatory (that is to say, a telescope and two photographic observers) devoted exclusively to this work, with an office and staff of measurers and computers located elsewhere, where the services of students and others could be secured, living is less expensive, and facilities for instrumental construction and repair are more accessible than in the southern hemisphere. To complete the work there must of necessity be a corresponding observatory in the northern hemisphere, and to complete the whole work in any reasonable time there should be several such pairs of observatories.

To make a beginning, so as to test the accuracy and probable value of the work thoroughly, it would be well to install one observatory of the kind in the most favorable situation and to confine the work, say, to four overlaping areas at each alternate hour of R. A. in each zone of 4° in declination and from declination 60° at each 4 hours of R. A. The results of such a series of pictures, taken and discussed, would lead to results of immense general importance, and would give some close approximation to the average parallax of stars of different magnitudes and proper motions, and would be an excellent pioneer program to ascertain the weak points of the original arrangements. It would not too greatly increase the program if plates having for their centers a number of the stars of more remarkable proper motion were added—indeed, perhaps the program might be best begun with these.

A, 4. I am hardly disposed to support the plan suggested by you of extending the zones on the plan of the *Astronomische Gesellschaft* from declination —32° to the south pole.

It is a far more accurate and useful plan to select the stars which are best distributed for determining the constants of photographic plates, as has been done at the Cape for declination —40° to —52°, and then to determine from the photographic plates the places of all required stars—say all stars to the eighth or ninth magnitude.

On the *Astronomische Gesellschaft* plan you get for this purpose an unnecessary number of stars in some parts of the sky and an insufficient number in others. For the zone —40° to —52° I found 8,000 stars ample, and they are as uniformly distributed as it is possible to select them. From the coördinates of our plates (and we are

about to publish zones —40° to —42°, the rest following in course of a few years) with the plate constants which will also be published any computer can construct a catalogue of all stars to the seventh, eighth, ninth, tenth, or eleventh magnitude, as he may see fit.

A, 5. The discovery and measurement of close double stars is an important branch of astrometry which is far behind in the southern hemisphere. I have great hopes that ere long a powerful telescope will be erected at Johannesburg for this purpose. Mr. Innes, recently my secretary, has been appointed in charge of the observatory there. At present his work is officially that of meteorologist, but I have great hopes that, having regard to his proficiency as a double-star observer, his enthusiasm and his power of exciting scientific sympathy, and the number of wealthy and large minded residents there, he will ere long be provided with a first class equatorial fitted for research on double stars. Meanwhile I propose to lend him the portable observatory I used at Ascension and a 6-inch equatorial of my own, which he is to employ in making an independent determination of the magnitudes of a number of stars on each of the C. P. D. plates.

But this prospect should in no way interfere with the erection of a second large telescope devoted to the same work, for independent comparison is at least as important in this department as in fundamental meridian work.

I had the satisfaction, when I visited Johannesburg last May, of selecting a site, outside of the town, which I have little hesitation in saying is one of the finest in the world for an observatory. It is nearly 6,000 feet above sea level, the atmospheric conditions seem to be most favorable, and on my recommendation the site, 10 acres in extent, has been secured by the government.

So much for astrometry. I agree with you that the provision made under the direction of Pickering at Arequipa and the labors of Roberts and Innes in South Africa sufficiently fulfill the requirements of photometric research as compared with that class of work in the northern hemisphere, and the work of the Carte du Ciel appears to be provided for.

Astrophysics.

B, 1. I have no hesitation in saying that what is required is the erection of the largest possible reflecting telescope for exact researches on the spectra and motion in the line of sight of the fainter

stars. What you want is an instrument that will collect the largest possible amount of light from a star within the jaws of a spectroscope slit.

Not only does a reflector provide this on a larger scale than it is possible to attain in a refractor, but it unites the rays of every refrangibility in one focus.

For spectroscopic work a Cassgrain reflector seems the best form to adopt, as the cone of rays, with its smaller angle of convergence to focus, permits use of a comparatively long collimator. The position of the spectroscope is also convenient, because, having regard to the weight of the speculum, the radius of motion of a spectroscope near the speculum end must be much smaller than that of one attached near the principal focus of the large mirror, and it is also much more convenient of access in the former than in the latter case.

Of course, photographs of nebulæ, &c., could be taken in the focus of the principal mirror, but this is a less urgent need than the spectroscopic researches.

With the modern ball bearings and electric-motor motions I see no difficulty in conveniently mounting a mirror of 6 or 8 feet diameter.

The erection of such an instrument in some very favorable position is, I think, the next great step that should be taken. We are in a position now, with refined and well studied apparatus, to attack the determination of motions in the line of sight of all the brighter stars ; but to get sufficient light to photograph the spectra of the fainter stars under such dispersion as will furnish reliable determination of motion in the line of sight requires a telescope of greater light grasp than I fear we shall ever get from a refracting telescope. For less money than the cost of a 40-inch refractor one could mount a reflecting telescope of twice that aperture—*i. e.*, of four times the light grasp—that would unite all the rays of light from a star in one focus.

I put this so far beyond all other demands of astrophysics that I make no further suggestion, and I do feel that steps should be taken to urge its fulfilment.

I believe that Bloemfontein, in the Orange River Colony, would be an ideal site for the erection of such an instrument.

[*From Dr. M. Nyrén, Wirkl. Staatsrath, Pulkova Observatory.*]

[Translation.]

PULKOVA, *August 14, 1903.*

The proposition for the observation of stars in the southern hemisphere, which you were so kind as to send me, interests me greatly; so much the more in reference to some points of the program, because we also here have for a long time cherished ideas tending in the same direction. As a small contribution to the realization of these ideas may be considered, the extension of the Pulkova Fundamental Catalogue to —30°, now attained by observations in Odessa. By numerous reference stars these observations are intimately joined with the Pulkova system. I send by this mail the program and the list of stars for this combined catalogue.

I quite agree with you that the first task for the observing station in the southern hemisphere should be to create a trustworthy fundamental catalogue from —20° to the south pole. In this catalogue the Auwers list of 480 stars, extended to the pole, should be included; but I would add thereto a number of stars, especially in those places where the stars of Auwers' catalogue seem to be brighter than may be desirable for fundamental stars, third magnitude and brighter. In this manner we should obtain a standard list of about 600 stars, in accordance with your proposition.

For the observation of the right ascension of these stars it would not be troublesome to find many places qualified for the purpose. With the declination it is doubtless more difficult; in this regard even the most southern portions of Australia are, as I think, too far from the poles. Besides, the observations made at the most southern observatory, Melbourne, are apparently affected by suspicious anomalies in refraction. In my judgment, the fundamental declinations should be observed on the continent of South America, as far as possible south of the —40° parallel, at a station where the ground to south and to north is as nearly similar as possible. Under favorable atmospheric conditions two observers could accomplish in two years a fundamental catalogue of 600 stars with 8 to 10 observations of each. For the great secondary catalogues, which demand a longer time, the observations, in case of need, could be made at another station, and also with another instrument.

As the number of fundamental determinations of the southern stars is rather small, the catalogue in question should necessarily

receive great weight. It is then, according to my opinion, the best economy of time and money to provide the observing station with equipment of the first rank. Your proposition that the same instrument should be employed alternately north and south of the equator will help us to eliminate many errors from the star places. Concerning the nature of the instrument, I should, for the declinations, in place of the meridian circle, prefer a vertical circle of moderate dimensions—5-inch aperture. The vertical circle seems to me, for a new observing station, preferable also from this point of view, that the consolidation of the instrumental piers, which for a meridian circle requires a long time, is for the vertical circle of no importance.

As corresponding in accuracy to the declinations determined in such manner, the right ascensions should also be observed with a transit instrument. This part of the work could without inconvenience be made at another station, at Cordoba, or in Australia.

For the observations of the stars of the secondary catalogues, the instrument described by you will certainly do good service.

I submit these, my ideas, to your judgment, and I should be glad if you find them worthy of any attention.

I have delayed the answer of your letter until Dr. Backlund's return from a journey in Germany.

[*From Director O. Backlund, of the Imperial Observatory, Pulkova.*]

[Translation.]

PULKOVA, *August 20, 1903.*

In order to make my answer to your valued communication more intelligible, I premise the following remarks:

Fundamental determinations of star positions stand in the first rank among the chief undertakings of the Pulkova Observatory. To this end W. Struve had the transit and vertical circle constructed, by means of which the positions of the so called Pulkova "Hauptsterne," 381 in number, have been determined in three series, namely, at the epochs of 1845, 1865, and 1885 (about). In the year 1894, in accordance with a plan by Nyrén, the program of the two instruments was enlarged by Bredichin so that about 1,000 additional stars of the fourth to seventh magnitudes should be determined for the epoch 1900, in general after the same program as for the "Hauptsterne." This series of observations is now completed. When I undertook the directorship, in

1895, I endeavored still further to extend the fundamental observations, and accordingly brought about the establishment of a branch observatory at Odessa (north latitude 46°) whose task it is to extend the limits of our fundamental observations to —30° of declination. In accordance with the experience at Pulkova during the course of sixty years, the transit and vertical circle were selected as the instruments best suited for making these observations. After thorough reflection I decided in favor of dimensions of these instruments smaller than those of the Pulkova instruments. Since stars fainter than seventh magnitude would not be observed, the aperture of both instruments was fixed at 4 inches and the focal length at 4½ feet. With dimensions so small the observations can be made far more conveniently, and with the vertical circle twice as rapidly as with the Pulkova instrument. The observations for a fundamental catalogue of the same extent as that of the Catalogue of the Pulkova "Hauptsterne" were begun on the third of February, 1901, and, so far as the number of observations is concerned, upon the same program as that of Pulkova. The work was completed exactly a year after beginning, while such a series of observations at Pulkova has never been completed hitherto within less than seven years. Of course, this rapidity of work was made possible not alone on account of the more convenient instrument, but also on account of the decidedly better climatic conditions. The observations are already reduced, and it proves that their accuracy comes up to that attained at Pulkova. Both at Pulkova and at Odessa Repsold's self registering micrometer is adopted. This autumn the observation of the Pulkova "Hauptsterne" is to be taken up for the fourth time, and for the epoch 1905. On this occasion the number of stars has been increased to 500. Simultaneously about 200 more southerly stars will be observed at Odessa, so that the resulting catalogue will contain about 700 stars independently determined between the North Pole and —30° of declination.

It will probably be clear, after this circumstantial description of our fundamental observations, that nowhere can the news of the purpose to institute fundamental observations of about 700 fundamental stars in the southern hemisphere awaken a higher interest than here in Pulkova. Indeed we are permitted to hope, in accordance with your grand project, that at no distant time the long desired and absolutely necessary fundamental observations will be extended over the entire sky, to aid in the further development of

our knowledge of motions in the planetary system and in the stellar field.

That the observations, especially those which concern the declinations, should be made, if possible, under a southerly latitude of at least —40°, I am in agreement with Nyrén. As to that which pertains to the planning of instruments, in consideration of the high degree of technical skill in the construction of instruments in America, it might appear venturesome to express decided opinions from here. I content myself with calling attention to the experience which we have gained here in Pulkova and in Odessa with the transit and vertical circle as to the determination of right ascension and declination with special instruments.

If, in addition to the fundamental determinations, the projected zone observations can also be secured, you will in that way earn the thanks of astronomers for all time.

There can be no doubt that the derivation of astronomical constants from observations in the southern hemisphere is a thing to be desired. For example, the very discordant values of the aberration constant, which the determinations secured at different observatories have recently shown, prove how necessary classic observations are. Latitude variations demand observations at places separated from one another as widely as possible. In accordance with an agreement with Potsdam, a series of observations for this purpose will be undertaken here in Pulkova, and with a zenith telescope of 5 inches aperture.

In regard to your remaining questions, so far as I may be permitted to judge, they are not less rationally proposed. You will be able to count upon the unanimous approval of the scientific world not less for these than for the others.

That the existing arrears in astronomical work in the southern hemisphere is felt as a drag at every step in every investigation now in progress cannot be denied. If on that account the investigation planned by you is realized, then a great drawback will be thereby removed; but the well known American energy affords a warrant that the carrying out of this plan will not be long delayed. A speedy realization of this project means an immense advance in science.

[*From Director E. Becker, of the University Observatory, Strassburg.*]

[Translation.]

STRASSBURG, *August 23, 1903.*

I take note of your letter of July 8 with the highest interest, and I fully assent to the opinions therein expressed. It is a fact not to be overlooked, and one originating in the distribution of observatories upon the earth, that our knowledge of the southern heavens is very much behind that of the northern hemisphere. So long as this inequality exists we must necessarily relinquish the idea of obtaining a satisfactory solution of many and indeed the most important cosmic problems. That this deficiency will be remedied within a time which we can now foresee by the establishment of new governmental observatories in the southern hemisphere is not to be expected, and an effort must be made—which would be greeted with the greatest joy in case of success—to induce private institutions to lend a helping hand. An observatory established in a favorable site, equipped with modern instruments, under skilled and energetic direction, with a staff of experienced observers and practiced computers not too small in number, in my judgment would be able to complete a work which would suffice to fill up the gaps in our knowledge that are now most deplored.

What tasks are deserving of the most prominent place on the program is a question which it is not entirely easy to answer from an objective point of view, and the answer would also depend upon the means that are available. On the whole, I am in sympathy with the arrangement set forth in your statement, and, for my part, would assign the preference to propositions 1, 3, 4, and 5, without underestimating the importance of the other tasks. As to 1, it appears to me worthy of consideration, whether upon grounds of economy, the undertaking ought not to be limited to stars of the sixth magnitude, or, in any case, to those of the sixth and one half, and whether the determination of position for stars fainter than the sixth or sixth and one half magnitude should not be assigned to the zone work, which, according to experience, is susceptible of producing very accurate results.

[*From Dr. Ralph Copeland, Astronomer Royal for Scotland.*]

EDINBURGH, *26th August, 1903.*

I have read your letter and inclosure of July 8 with deep interest. Regarding the various classes of special observations to be made in the southern hemisphere, included in your statement, it seems to me that classes 1, 4, and 3 are of the greatest immediate importance, and their urgency is probably in the above sequence, the first being the most important.

Class 2 I consider of relatively far less immediate interest, seeing that the results arrived at are by no means of the fundamental character of those obtained by classes 1, 4, and 3.

Respecting class 1, it is conceivable that the observations might advantageously be divided into two groups: *a*, the fundamental determinations of the places of 600 principal stars, together with the essential observations of the sun; *b*, the precise observation of the 5,400 remaining stars brighter than the seventh magnitude between —20° and the south pole.

Group a.—Possibly this work could be most satisfactorily accomplished by using two instruments—a vertical circle and a specially efficient transit instrument. But these instruments, and in particular the cells of the object glasses, should be made of mild steel, which has a coefficient of expansion differing but little from that of glass and much smaller than that of brass, hitherto so largely used in the construction of astronomical instruments of precision. With the lenses held by springs on the plan designed by Fraunhofer, it is probable that they would rest almost absolutely immovable in their cells in all positions of the instrument, and that their minute real movements would be directly related to changes of temperature.

For the vertical circle I would suggest the trial of a design that occurred simultaneously to the late Dr. Common and to myself. It consists in placing an object glass at each end of the tube in such manner that the focus of either object glass shall fall absolutely on the outer surface of the other. Spiders' webs are to be replaced by fine lines, engraved or etched on the outer surface of each object-glass (I have seen lines of this kind not appreciably inferior to the finest natural webs). This construction would permit of the "end for end" reversal of the vertical circle, as well as of the ordinary "right and left" reversal, thus eliminating flexure from every determination of double zenith distance. I pass over the obvious

details of the light swing-frames with their counterweights carrying the eye piece at each end of the telescope and readily turned aside when not in use. I feel assured that such an instrument in the hands of a skillful observer would yield zenith distances of an accuracy not yet attained. One of our first opticians assured me that by the use of twin discs of glass and alternate grinding there would be no difficulty in producing the requisite pair of object glasses.

The transit instrument should be reversible on every object. It should have a clean drawn cylindrical tube of mild steel, attached in the simplest efficient manner to the middle of the enlarged steel axis through which it passes. This kind of tube was suggested by Sir David Gill in conversation many years ago. The transit to be taken by means of a movable recording wire—say, 10 seconds in each position of the instrument—by the well known method used so largely by Professor Albrecht and his staff in Germany. By this method collimation error and inequality of pivots are at once eliminated, and, as it seems to me, the troublesome magnitude equation is practically evaded. Moreover, it is certain that the personal equation is confined within extremely narrow limits. As it would be necessary to observe the sun with this instrument, it would probably be desirable to use a reversing eye piece; but experience would doubtless soon show whether this is desirable or not.

Group b.—This work—the precise observation of the 5,400 remaining stars brighter than the seventh magnitude between $-20°$ and the south pole—could probably be rapidly and efficiently accomplished by the use of a meridian circle, which I should like to see made of steel, with the graduations on gold, platinum, or on an alloy of gold and palladium; but by no means upon silver, which is so liable to tarnish and necessitate risky cleaning.

The observations in class 4 are well worth undertaking with the least practicable delay, as they will gain largely in value by every year that elapses after they are once secured. By carefully boxing in the circle of the instrument used and securing an efficient circulation of the air confined within the box, it is probable that the accuracy of this class of observations could be measurably increased. Class 2 seems to me to be the least important part of the proposed undertakings, the resulting stellar distances being apparently peculiarly mixed up with those of the few available comparison stars. But may we not hope, now that the displacement of the earth's axis of rotation with regard to the observatory can be taken into account, that the fundamental observations of class 5 may begin to indicate

the annual parallactic displacements of all the nearer stars relative to the bulk of the 600 stars under observation? In course of time it would then be necessary to take these parallaxes into account in determining the apparent places of the stars affected by them.

Class 6 evidently calls urgently for a large refractor of the most perfect kind, used at a station where the definition is of the very best. Possibly this work could be best carried on at the proposed high level astrophysical observatory, although I may mention that at Jamaica, in the end of 1882, I found the definition near the level of the sea exceedingly good on many nights. In that island it would be possible to find a perfectly salubrious station at an elevation of 4,000 to 5,000 feet, and quite probably equally favorable localities could be found in some of the more mountainous South Sea islands.

The general sidereal part of your scheme ought, as you say, to be carried out south of latitude —30°, but in the southern hemisphere the climate increases in severity at relatively moderate distances from the tropics much more rapidly than it does north of the equator; hence every care should be taken to avoid a site too far to the south. At the same time I most earnestly support your view that no pains should be spared in choosing a thoroughly salubrious climate; otherwise the most devoted members of the expedition will be just the most likely to fall victims to any error of judgment in this most important matter.

In conclusion, I would suggest placing your most important timekeeper in a partial vacuum—say, under nine tenths of the normal local atmospheric pressure. This is easily secured in a cast iron box with a three-quarter-inch glass face resting on a slip of rubber. A "quicksilver sleeve" permits of winding the clock twice in the week. A small syringe removes any slight leakage of air. We have found here that the air in the box must not be dried artificially, or the oil necessary to the clock work will decompose and the clock will stop.

A great improvement would be to add an outer case, the air in which, by a simple electric contrivance, could be kept at a uniform temperature slightly in excess of the highest temperature likely to occur naturally. Under these conditions, any well made clock ought to have a rate subject only to very minute changes.

[*From Dr. W. H. M. Christie, Astronomer Royal, Greenwich.*]

GREENWICH, *August 29, 1903.*

I fully agree with the Committee that a much larger provision for astronomical observations in the southern hemisphere at the present time is desirable, and that with this object a Southern Observatory of an expeditionary character for definite pieces of work which could be completed within a limited number of years should, if practicable, be established.

Taking the special observations regarded as important by the Committee:

1. The proposed fundamental determination of star positions would be of great value. With this should be combined observations of the sun and determination of position of the ecliptic.

Observations of the moon, as nearly continuous as possible *throughout the lunation*, for several years are very much needed for the improvement of the lunar tables in regard to terms of short period, and these might well be combined with the observations of fundamental stars.

Observations of the planet Mercury are also much wanted.

2 and 3. I fully agree as to the importance of these and the need for making provision for them.

4. This does not seem to me so much needed at the present time. The observation of reference stars for the plates of the astrographic catalogue now being carried out at Cordoba, the Cape, Sydney, and Melbourne largely covers the ground, and though the number of stars is less than on the plan of the *Astronomische Gesellschaft*, the place of a far greater number will be determined from the photographic plates with an accuracy greater than that of meridian observations.

There is, however, a gap in the southern zones for the astrographic catalogue, viz., zones —32° to —40°, the plates for which are being taken at the Perth Observatory (West Australia), but with little prospect of their being measured there or of the necessary reference stars being observed. Another zone, —17° to —32°, undertaken by Montevideo, is in even worse condition, the funds for providing the photographic telescope not having been granted as yet, though promised by the president of Uruguay.

In place of 4, I should prefer to substitute the completion of the zones of the astrographic catalogue by the taking and measurement of the plates and the observation of the reference stars for the zones

not otherwise provided for. This seems to me to be urgently needed, and the work could be completed in a limited number of years.

5 and 6. I quite agree as to the importance of these.

7, I presume, may be considered later.

In the matter of location, I would submit that New Zealand and Tasmania should be carefully considered. An observatory established in New Zealand would have a good chance of being taken up by the government, as in the case of Cordoba, after its specific pieces of work were completed, and it would undoubtedly give a great stimulus to astronomy in the colony.

[*From Professor J. C. Kapteyn, Director of the Astronomical Laboratory, Groningen, Holland.*]

VRIES (NEAR GRONINGEN), *August 31, 1903.*

In answer to the valued invitation of your Committee, I will unreservedly state my views, though they may seem somewhat radical on some points.

For evident reasons there cannot be the slightest doubt that a southern astronomical observatory can do much more for the promotion of astronomy than a northern one.

The works falling in the first line for cultivation at such an observatory I consider to be:

1. Determination of stellar parallax.
2. Fundamental determination of right ascension and declination.
3. Determination of radial velocities of the fixed stars.

There are, most certainly, several other works which urgently call for execution, but I think the three works mentioned must take precedence of all the others. Moreover the chances of these other works being undertaken elsewhere on a more or less sufficient scale seem to be somewhat better. So, for instance, the extension of the astrographic catalogue from declination $-32°$ to $-90°$ in your "confidential statement."

At the Cape the positions of the reference stars for the "Carte du Ciel" have been determined by the meridian circle. This being so, the positions which will be obtained by the measurement of the plates (as soon as they shall have been reduced to right ascension and declination) will make the want of an extension of the astrographic catalogue little felt for that zone.

It seems but reasonable to hope that the existing southern observatories (Cape, Melbourne, Cordoba) will cooperate to furnish the

same material for the reduction of the remaining parts of the southern sky. Therefore, though (with a view to the indefinite time which may still elapse before we get a complete catalogue of right ascension and declination for use from the "Carte du Ciel") I would most cordially rejoice in an extension of the astrographic catalogue, I would still place this work in the second rank of the works most urgently demanded at the present moment.

However this may be, I will restrict my remarks to the three works mentioned, which are the most important of all.

(1) *Determination of Parallax South of Declination —20°.*

As, in my view, there is at present no work so urgently demanded for the advancement of astronomy as the determination of parallax on an extensive scale, the equipment of the observatory for this purpose should be as complete as possible; for instance:

a. Two photographic telescopes, say of 40 cm. aperture and 6 meters focal length. They ought to give round images over a field of 2° diameter.

b. One telescope of 40 cm. aperture and a focal distance as great as is compatible with rigid mounting. A round field of, say, 80′ diameter or even somewhat less will be sufficient.

c. One transit instrument of 7 inches opening.

d. One heliometer of 7 inches.

Regular morning and evening observations demand two observers for each instrument. A small part only of the time of these observers would be taken up by the observations; the rest would be devoted to the measuring of the plates and the reduction of the observations.

To provide such an outfit and such a staff exclusively for the purpose of parallax determination may seem extravagant. I do not think so. The need of a better and more solid knowledge of stellar distance is so great that we should stick to some such plan as is involved in the above, even if it appeared that thereby the funds available would be exhausted. If something must be sacrificed, I think the instruments *c* and *d* could be best dispensed with, as it seems more likely that the observations to be made by these instruments will be taken up elsewhere.

The two telescopes *a* would serve for a photographic Durchmusterung for parallax. Elsewhere (Publications Astr. Lab. Groningen No. 1, pp. 87–98) I have explained at some length the feasibility and desirability of such a plan.

Notwithstanding the reasons adduced (pp. 97 and 98), some astronomers still think that we ought to restrict ourselves to the brighter stars and those of considerable proper motion.

We may now exclude the former from consideration, because, for obvious reasons, a photographic Durchmusterung will do little for the very brightest stars. They must be treated by instruments *c* and *d*, and there will be little difference of opinion as to the desirability of investigating as many of these stars as may be possible.

The real question thus is: Has the time come to make a complete Durchmusterung of parallax for the fainter stars (say 6.0 to 10.0)? Or may we restrict ourselves to stars of sensible proper motion only?

It will be a relatively small undertaking to obtain the parallaxes of the 200 stars of greatest proper motion (this is about the number of stars with proper motion exceeding 0″.6, known at present, a great part of which are bright ones).

It seems not too much to expect that these will be observed elsewhere. In fact, I think the greater part of them have already been measured. For the rest, if necessary, they will be dealt with by the instruments *b*, *c*, *d*.

Setting aside also the consideration of these stars, therefore, it remains to answer the question: What are we to do afterwards—after the observing of the 200 stars of greatest proper motion?

The more important part of our aim must be to get a knowledge of the distance of a great part at least of our nearest neighbors in the universe, in order that we may begin by making a study of the laws in their distribution and motions. Now, if from the very beginning we exclude all the stars of which the proper motion at right angles to the visual ray is small (and we virtually do this by confining ourselves to stars of great proper motion), then we may foresee at once that the finding out of any real law in the motions will be impossible. Our aim will be defeated from the very outset.

There are other considerations more amply set forth in the paper quoted which must lead to the conclusion that for a study of the construction of the stellar world we cannot escape the necessity of making a Durchmusterung for parallax. It would be a noble task for the observatory to be erected to take the lead in such an undertaking.

For a fairly fine climate I estimate that the two telescopes *a* together would furnish a duplicate set of plates for the whole sky from declination —20° to declination —90° in about eight to ten years.

All the objects found that are suspected to have a fairly large parallax would be taken up by telescope *c*. They would be further investigated only in those cases in which a first plate confirmed the large parallax.

On the plates furnished by this telescope only the principal object, with five or six well chosen comparison stars, would be measured. The work of measuring and reducing would be very moderate, therefore, especially as a reduction with three constants would be sufficient in nearly every case.

Instrument *c* would serve for the parallaxes of the brighter stars (say 0—6) and, together with instrument *d*, for the further investigation of objects of certainly measurable parallax.

The observatory ought further to be fitted out with (say) ten measuring machines. It is very probable that the observers, though their labors at the telescope would take up only a small fraction of their time, will be unable to make the measurement and reductions keep pace with the production of the photographs. It will be necessary, therefore, to procure assistance for them. This, however, will be a question of cheap labor. (See P. S.)

I have dwelt thus long on the subject because it is practically a new one. On the others I have only a few words to say.

(2) *Fundamental Determination of Right Ascension and Declination.*

The great importance of a fundamental determination of star positions, with extension of these observations by secondary methods to include every star brighter than the seventh magnitude south of —20°, is evident to any one who has made some study of stellar motion.

I feel very warmly for the plan of transporting to the southern hemisphere, for a short term of years, one of the reversible meridian instruments of the northern hemisphere. The observations of a considerable number of stars with the same instrument at a northern and at a southern observatory cannot but lead to a material reduction of the influence of systematic instrumental error and error of refraction.

It would seem to me that it would be advantageous to make the determinations of right ascension and declination by two separate instruments, a transit (for which the instrument also used for parallaxes may serve) and a vertical circle. Besides other well known reasons, there is this one: If for the determination of declination we bisect the image of a star by the horizontal wire, its

brightness is very considerably reduced, especially in the case of the somewhat fainter stars. Thereby the personal error depending on brightness must be changed and an element of uncertainty introduced into the right ascensions. If I am not mistaken, the effect was found to be quite perceptible in Leiden.

As the elimination of systematic error influenced by refraction and flexure is of such paramount importance for our fundamental declinations, I would suggest to supplement the above determinations by the determination of some hundreds of fundamental declinations by the method explained in Copernicus III, pages 147–182, which make the result absolutely free from both refraction and flexure. * * *

(3) *Determination of Radial Velocities.*

I have no suggestions to add to the plan developed in the "confidential statement," with which I most cordially agree, as far as I can judge of the matter.

P. S.—In regard to the measurement and reduction of the plates for a Parallax Durchmusterung, which might perhaps be considered to put too heavy a strain on a single observatory, I would like to add that no doubt many of the smaller observatories, not too well provided for, would be only too glad to do the work of first class importance by measuring and reducing good parallax plates. Under certain conditions the laboratory at Groningen would very willingly undertake the complete work of measuring reduction and description for a duplicate set of plates for a zone of 2° width at least.

[*From Dr. Arthur Auwers, Secretary of the Royal Prussian Academy, Berlin.*]

[In a private letter to the chairman Dr. Auwers discusses various topics. The following extracts refer to the plan for a proposed observing station in the southern hemisphere.]

[Translation.]

GREIFSWALD, *October 6, 1903.*

The "confidential statement" enclosed in your letter of July 7 designates the most pressing astronomical tasks to be worked out in the southern hemisphere so fully and, according to my judgment, so much to the point, that I find very little to add thereto; and, furthermore, my pressing work has hindered me until now from pre-

paring an answer. You wish me to express myself especially in regard to Nos. (1), (2), and (4), and, although belated, I will do this.

The *most important* task that today confronts the southern observatories is, in my judgment, the production of *really fundamental* determinations for a selected list of stars. Such determinations for the southern hemisphere are still wholly wanting. We are hoping for a series of such within the next few years from Gill, but it is of the highest importance that we shall be in possession of *several* such determinations, homogeneous and each as a check on the others, and the establishment of a temporary observing station for this purpose would therefore be a timely undertaking.

The employment of a meridian circle, which should be used before and afterwards for similar determinations in the northern hemisphere and which has proved itself to belong to the first rank of first class instruments, would be wholly worthy of commendation. By this means one would at least, to a great extent, remove one element of uncertainty, the adopted flexure. It is possible that the advantage from the employment of this device will not be so great as you apparently hope, since the principal source of uncertainty in our declinations arises from the uncertainty of the refractions, in which local anomalies remain, arising partly in the observatory and partly in its surroundings, and which can be rendered less and less harmful in their effects through increase in the number of observing stations.

The attempts which have hitherto been made to establish an absolute system of declinations through comparison of observations made in opposite hemispheres are founded on the supposition that the refractions on both sides of the zenith are alike, and I doubt whether this supposition is correct for the majority of those observatories upon whose observations we have had to rely up to the present time for the establishment of systems of declination. The correctness of this supposition seems to me especially doubtful in relation to the two southern observatories which, up to the present time, have afforded the most accurate places of the brighter stars. The Cape, as well as Melbourne, observatories have the ocean to the south and a heated continent to the north, and over these different regions there may be very differently arranged masses of air. I consider it, therefore, very important that the new observation station should not have a similar position, but that it should be either purely insular or purely continental. At the same time, the southern-

most latitude possible is desirable. Perhaps the southern part of the Argentine Republic offers what is, on the whole, the most practicable compromise (also the possibility of existence for several years).

As closely related to the fundamental determinations and as a work to be accomplished with the same instrument it is very desirable, as you propose, that there should be complete observation of all stars south of —20° and to the seventh magnitude, inclusive, four times for each star (equally distributed in the two positions of the instrument, and preferably according to the example set by the Pulkova series for 1855, with exchange of objective and ocular). In accomplishing this the period of observation will be scarcely lengthened and a second work of the first importance would be produced.

Of still greater importance than this second work, attached to No. 1, is the continuation of the astrographic zones, the proper and speedy observation of all stars down to the ninth magnitude upon the southern sky (No. 4). In Cordoba this work would have been extended beyond —32°, but since Dr. Thome has now undertaken a part in the photographic chart it is very desirable that others should undertake the continuation of the zones. For this purpose, in addition to the meridian circle for No. 1, a second instrument would naturally be required; but I do not see the use of giving to this instrument a construction of the form you have in mind. The ordinary meridian circle is entirely suited for zone observations, and in any half way favorable climate with such an instrument two observers (one at the ocular and the other at the microscopes) could with ease make 10,000 observations annually at less than —40° of latitude where one has sufficiently long nights at all seasons; two sets of observers could make 20,000 observations annually.

No. 2 is also a task of great importance. The determination of the mean distance of the stars of various orders of magnitude is necessary, in order to provide a firm foundation for investigations into the structure of the stellar universe. But it appears to me impossible to reach this result otherwise than when one investigates the parallax of each single star of the brighter orders of magnitude, together with a sufficient number of the following orders, as far down as the means of measurement will permit. Dr. De Ball has planned such an undertaking and is corresponding in relation to it with others who have heliometers at their disposal—among others with Dr. Elkin. Therefore I will not go into further particulars, and will only remark that it goes without saying that it would be

of the highest value to secure for this enterprise the cooperation of another southern observatory in addition to that of the Cape, this also to be provided with a 7 or 8-inch heliometer. In my opinion, this is the only instrument with which, up to the present time, one has been able to secure reliable determinations of parallax.

* * * * * * *

With these remarks I have desired to express my personal and lively interest in the plans of the Carnegie Institution, for which I wish a speedy and complete fulfilment. I leave to you to communicate to your fellow members of the Committee so much of this letter as you think best, but otherwise I desire that you will consider it a private answer to your communication.

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II. CORRESPONDENCE RELATING TO PROPOSED SOLAR OBSERVATORY.

INTRODUCTION.

In January, 1903, a confidential statement regarding a proposed solar observatory was sent by the Secretary of the Committee to a number of astronomers and physicists. This letter stated that the principal purposes of the observatory, as they then appeared to the Committee, would be to investigate (1) the intensity of the solar radiation and its possible changes during a sun spot period; (2) the problem of the solar constitution, through observations with the spectroscope and other instruments; and (3) various stellar and nebular problems connected with the evolution of the sun and stars. The necessity of choosing sites especially suited for such work was also pointed out, and the suggestion was made that for the study of the solar constant a high mountain station, with a second station near the base of the mountain, might be required. Suggestions were requested regarding the proposed program of work, the selection of sites, and any other subjects connected with the observatory.

In response to this letter the following replies were received:

LETTERS FROM CORRESPONDENTS.

[*From Professor C. A. Young, Director of the Observatory, Princeton, N. J.*]

FEBRUARY 7, 1903.

Naturally I am very much interested in the question of a special astrophysical observatory. There is no question that the lines of

research indicated in your "confidential" paper are important and ought to be followed up in some concerted manner; but I own to some doubt whether it would be best that all that research should be concentrated at any one, two, or even three stations. Cooperation between workers widely enough separated to secure nearly continuous observation might be better, unless some locality can be found where observations are practicable *all the time*, and I know of no such locality, in the United States, at least. Still it is obvious that, given the "ideal" director with adequate means at his disposal, there would be great advantages in the concentration, perhaps quite sufficient to overbalance the disadvantages.

As regards my experience at Sherman, it did not indicate any advantages as to average aerial conditions over Hanover and Princeton, but during my six weeks' stay there (I think it was six weeks) there were two or three magnificent nights, when the conditions were better than I ever saw them here (one night here, perhaps, excepted, or rather a few hours that night). For solar observations, however, the conditions from half an hour after sunrise till 9 or 10 a. m. were fine more than half the time. About 11 it usually began to cloud up, and in the afternoon thunder storms were in order till 8 or 9 p. m., and for some hours afterward the air, though very transparent, was very unsteady. The seeing may have been good after midnight, but I did not examine it often, as my work on the sun gave my eyes all I could safely do with them.

Of course, my statement as to the behavior of the weather can not safely be taken as applicable to all years and months. It was in the months of July and August, 1872, that my observations were made, and I remember that some of the few residents of Sherman said that the conditions were unusual for those two months on account of the unusual amount of snowfall the preceding winter on the mountains west and south of Sherman; but from all I can learn I should think there was much more likelihood of finding better average seeing not very far from the sea, as in southern California.

I ought to add that undoubtedly an outburst of vigorous solar activity on the sun's limb from August 1 to 6 had a great deal to do with my success in finding new chromosphere lines. During the first three weeks of work I made little headway and was much discouraged. When I began to get up and go to work at 5 or 5.30 a. m. things went better, but the days I have mentioned, August 2–5, gave me fully half my harvest.

[*From Professor Henry Crew, Director of the Physical Laboratory of Northwestern University, Evanston, Illinois.*]

FEBRUARY 10, 1903.

I have not earlier replied to your circular letter of the 30th ultimo for the reason that I have nothing of value to contribute in the way of suggestions. Congratulations are certainly due to the astrophysical world on the splendid prospects set before it by the Carnegie Institution.

My own experience, my reason, and my reading all lead me to think that you are not likely to put too much emphasis upon the necessity of untying the bundle of sticks before you attempt to break them. By which I mean to say that you are hardly likely to find at any one station the best conditions for undertaking more than one of the three problems which you outline.

The best conditions for the solution of any of these questions would seem to me something like the following:

1. A carefully and intelligently selected site to which an investigator might go with confidence.

2. A single, definite, and not too general problem; at least, a single problem at a time.

3. The selection of two men whose interest and ability in the matter no one doubts.

4. A simple, plain, but adequate material equipment in all except the central and essential instrument, and then make this the most powerful and most efficient in existence.

5. Study the men you put in charge, see that they are comfortable, but not "too comfortable," and, above all else, see that the conditions (mechanician, etc.) are such that these men's time can all go to the problem in hand. In other words, leave your men "foot free," and then hold them responsible either for results or for difficulties which are certainly insurmountable.

6. Energy without haste; test men and sites deliberately.

Of all the problems which you mention, the most pressing appears to me to be the need of a continuous record of what is going on at the solar surface. Psychologists often find abnormal cases the most instructive. It may be so with solar studies. Next most important appears to me the horizontal telescope of large aperture. I think this deserves a fair trial in the best attainable spot on the globe, both for spectroscopic and for photographic work.

[*From Professor E. F. Nichols, Director of the Physical Laboratory, Dartmouth College, Hanover, N. H.*]

FEBRUARY 12, 1903.

Your letter of January 28, concerning plans and projected work for a new national observatory, has been received. The plans for work embodied under the three heads in the letter seem to me admirable, and to include work of the most valuable kind yet to be done in astrophysics. My own work in astrophysics has been very limited, as you know, but the outlook and far reaching extent of the projected work for the new observatory seems to me in its variety to include nearly everything at present worth doing.

In particular the conditions required for the most successful measurement of the heat radiation of the brighter stars would be a clear and quiet atmosphere for night work, and a large concave mirror of at least 5 feet aperture. The mirror must be so mounted that the beam from the mirror will be reflected in a fixed direction, so that the heat measuring instrument need not be moved in following the star. It is further very desirable, if not absolutely necessary, that the radiometer or other heat measuring instrument may be surrounded by constant temperature conditions during observations. The results which might be expected from such an equipment I have already discussed in my paper in the *Astrophysical Journal*, vol. 13, p. 138.

I shall be only too glad to do anything I can to further the plans of your committee, and only wish that any suggestions that I might make could be based on a broader experience in practical astrophysical work. If at any time I can be of use to your committee in any way, I hope you will have no hesitation in calling upon me.

[*From Sir William Huggins, Tulse Hill Observatory, London.*]

FEBRUARY 17, 1903.

I am very glad to hear that there is some prospect of establishing new observatories to provide for observations and researches which require special conditions of position or of equipment.

The lines of work sketched out in your letter appear to me to be admirably thought out, and indeed so complete from the point of view of the investigations which have the more immediate claims that I do not see that there remains much, or indeed anything, for me to suggest.

I think that it is of first importance to have a permanent observatory furnished with a large *reflecting* telescope and a complete equipment of auxiliary instruments for astrophysical research on some site with the most favorable conditions of atmosphere. If this were near the equator, it would command the richest regions of both hemispheres. I mention this point in case it might not be found possible to build a separate southern observatory.

The observatory next in importance, it seems to me, would be one on the top of Mount Whitney, devoted especially to solar work, or chiefly so. I think the photographic method of getting the corona should be tried. Theoretically it is certain of success, if only the atmospheric conditions are but a little better than normal surface ones.

I suppose work could only be carried on during the summer, but if the conditions are as good as the altitude would suggest, there is certainly work enough for many years to come.

The observatory at the base might be regarded as temporary, and perhaps might be given up when a sufficient number of observations simultaneous with similar observations at the observatory on the top had been made.

[*From Professor Arthur Schuster, Director of the Physical Laboratory, Owens College, Manchester, England.*]

FEBRUARY 18, 1903,

In answer to your letter of the 28th January: I should, of course, be highly pleased if funds were to become available for the important work sketched out by you. Taking the different points of your letter in order:

1. I have recently looked carefully over a good deal of literature concerning solar radiation, and I confess I have not been impressed by the probability that simultaneous observations at high and low altitudes will help us very much. The differences which such observations could show would all be due to the layer of air included between the two levels. On different days the atmospheric conditions of that layer may be very different, and yet the atmospheric conditions at greater heights than, say 15,000 or 20,000 feet, might be the same. You would at once have errors introduced, and the observations at the high altitudes might by themselves give you better results than the combination. You must face the fact that it is impossible altogether to eliminate changes in atmospheric condi-

tions. Supposing that you were to come to the conclusion that the solar constant observed, even at high altitudes, varies with a sun spot period, it would still be open to doubt whether the change is not due to something that happens in the higher layers of the atmosphere. That, in my opinion, would be the most probable explanation, but as even this fact would be important to establish, I quite agree that observations on the solar constant should be made at frequent intervals.

Whether there are any actual differences in solar radiations at different times is a question that will, I think, be solved in a different manner. It seems to me exceedingly unlikely that any increase or diminution in solar radiation can take place equally and simultaneously all over the solar surface. If sun spots have anything to do with it, we must imagine the changes to come out differently or at least at different times in different solar latitudes. I would therefore consider it a matter of first importance to improve observational methods as much as possible, so as to be able to compare with the utmost accuracy different parts of the solar surface in different portions of the solar spectrum. A change in temperature of even 100° ought to make an appreciable difference in the radiation at the violet end, though the radiation in the red will not be affected nearly as much. I have been very much struck with a recent paper by Mr. Wood, of Baltimore, which describes a screen that absorbs the visible light, leaving the ultra-violet. I should say that simultaneous photographs taken of the solar disc by the ordinary method and with this screen might give interesting results.

2. I think you know already the great importance I attach to the careful investigation of the spectrum of sun spots, and the other points you mention are of equal interest.

3. Here again I agree with you that such work as the measurement of the heat radiation of stars and investigations of spectra with very high dispersion are bound to lead to important results, and if your present atmospheric conditions at available observatories are not sufficiently good it would no doubt be highly advisable to have a site specially selected for the purpose.

I am not, of course, able to judge whether the same site may be suitable for the solar and stellar work, and I do not want to discourage altogether the possibility of a station at the base of a mountain on which solar observations are taken. Some useful results might be obtained in this way, but I should not hope for very much and should not personally be inclined to recommend any great ex-

penditure for such a purpose. Possibly a very moderate equipment at the base would do all that is desirable.

Might I also mention, in addition to the matters you speak about, that a repetition of Dunér's work on solar rotation in the reversing layer is, I think, called for? I do not think the last word has been said in that matter.

[*From Professor H. C. Vogel, Director of the Royal Astrophysical Observatory, Potsdam, Germany.*]

[Translation.]

FEBRUARY 19, 1903.

In reply to your kind letter of January 15 in regard to the establishment of a great astrophysical observatory with stations at particularly favorable points, I wish to reply that I can only commend in the warmest way the realization of this great undertaking in the interests of science, and that I regard the investigations proposed by you, on the subjects named below, as so suitable and exhaustive that I am unable at present to suggest anything further:

(1) Solar radiation problems, involving the measurement of the solar constant at frequent intervals throughout the sun spot period.

(2) Solar investigations, principally of a spectroscopic nature, which require atmospheric conditions and instrumental facilities superior to those hitherto employed.

(3) Various stellar and nebular problems, such as could be undertaken to the best advantage with the aid of a large reflecting telescope.

In Europe we are not so fortunately situated as to be able to find so easily sites from which the proposed observations could be carried out to advantage. We can therefore only wish success to our American colleagues if the Carnegie Institution should provide the means necessary to carry out such comprehensive investigations, which are so important for astrophysics.

[*From Professor H. Kayser, Director of the Physical Institute, University of Bonn, Germany.*]

[Translation.]

FEBRUARY 20, 1903.

I am so busy at the end of our semester that I can reply only briefly to your letter of January 30.

If a new institution, provided with unusual instrumental means, is to be established for the study of the sun, it should have the solution of one especially important and fundamental problem as its principal purpose. The most important question seems to me to be with regard to the constitution of the sun. We seem to be infinitely far from the solution of this question—whether it is wholly gaseous or in part liquid or solid. This question must be solved through a very detailed study of the various parts of the sun. It seems to me particularly important to make a study of the sun spots. If their spectra could be photographed in the greatest possible detail throughout a solar cycle with large gratings, an important advance would probably be made. In this way it might be possible to form a proper estimate of Julius's theory—either to confirm or refute it. This investigation is a rather thankless task, since it consists only in gathering observational material; but a result drawn from it would be far more important for our knowledge of the heavenly bodies than the measurement of the radial velocity of a few hundred stars or the intensity curves of variables. As soon as the characteristic features of our sun are thoroughly understood, many other phenomena will explain themselves.

I would advise the use of plain gratings for this investigation, since freedom from astigmatism is necessary; the lenses should be preferably of quartz-fluorspar, and the solar image on the slit should be of large diameter.

Terrestrial spectra must, of course, be employed for the explanation of the phenomena. The laboratory must, therefore, be supplied with concave gratings of various numbers of lines to the inch and various radii of curvature; also with direct current for arc lamps and alternating current for transformers, as well as induction coils of various dimensions up to one meter spark length, in order that the spectra of the elements may be studied under the most varied conditions.

I have touched upon only a single question, which seems to me to surpass all others in importance. It goes without saying that a large institution would also undertake investigations requiring night observations. I name here briefly only two of investigations which seem to me particularly important: the spectra of the planets, in order to determine the nature of their atmospheres, and the spectra of a number of the brightest stars with a precision approaching as closely as possible that of Rowland's solar spectrum, so that their chemical constitution can be accurately compared, and if differences

are found in the spectrum of the same element the cause of these differences may be investigated.

I believe that the problems which I have named have hardly received serious consideration hitherto. All are for the purpose of determining accurately the physical and chemical constitution of at least a few of the heavenly bodies. In the case of the sun itself Rowland's atlas and tables may be regarded as only the first though a most important step toward a knowledge of its chemical constitution. A further study of the spectra of the elements would certainly permit 90 per cent of the lines now designated as unknown to be identified.

[*From Dr. W. E. Wilson, Private Observatory, Daramona, Ireland.*]

FEBRUARY 24, 1903.

I was very glad to receive your letter of January 28 and to see that there is the prospect of founding a large observatory for investigations on solar radiation and kindred subjects. I am sure such research can be profitably carried on only in situations such as you propose. Ireland is certainly *not* one for such work. I have been experimenting for two years with a new recording bolometer, which I think would give excellent results if it were mounted in a situation where it would get some sunshine. This it does not get here. If you would care to have it tried at your new observatory I would be delighted to send it to you. All you would require would be one of Callendar's electric recorders to work with it. It consists of two flat coils of platinum wire blacked and sealed up in an exhausted glass tube. This is enclosed in a brass tube with suitable diaphragm, so that one coil only receives radiation from the sun and a small bit of surrounding sky. The other coil is in the shade. These form the arms of the Wheatstone bridge of the recorder, and it gives a continuous record of the radiation. With the form of receiver designed by Callendar the coils were not in vacuum, but merely covered with a glass shade. One coil was black and the other bright, and both lay horizontally, so that the sun was never normal to them, and of course changing from hour to hour. I found that with this old receiver it was quite impossible to calibrate the curve with Ångström's pyrheliometer. Even on a dark, wet day it gave a considerable deflection, by reason of the glare from the clouds. With my new form the curve can be calibrated with the Ångström instrument perfectly, and by means of a planimeter, which is attached to the pen of the re-

corder, you can read every day the area of the resulting curve. I enclose you some sample records. Those marked A are taken with the old receiver and B with the new one. You will see that in B when there is no sunshine the curve falls to the zero line. In A it is always above it.

I wish you could also see your way to carry on a series of observations by allowing an image of the sun to transit over the aperture of a small radiomicrometer and recording the deflection on a moving photographic plate. By a discussion of the resulting curves I think it would be quite possible to determine whether the depth of the absorbing layer on the sun varies during a sun spot cycle. I began taking curves here, but the weather was so hopelessly unfavorable that I gave it up.

[*From Sir Norman Lockyer, Solar Physics Observatory, South Kensington, London.*]

FEBRUARY 25, 1903.

In response to your letter of January 29 regarding the work to be done by a new astrophysical observatory, I would say that your scheme practically covers the whole ground. There are, however, one or two points to which attention might be drawn:

1. Would it be advisable to erect a permanent building on a mountain summit without first putting up a temporary building and making observations from it for a year or two?

2. It is of great importance that the observatory should not be too far away from some large town easy of access, as modern work requires the investigator to be in touch and personal contact with scientific men for purposes of mutual assistance and advice in addition to the reasons given in your letter.

3. It is important that the laboratory equipment should be complete, for it is the mutual work of the observatory and the laboratory which helps the investigation.

4. The Janssen-Hale-Deslandres method of photographing the solar prominences should undoubtedly be undertaken and made strict routine work.

I am very glad to see that you mention the photographing of the ultra-violet end of stellar spectra, as this is important and we are at work upon it, although we have very small means here for carrying it on.

[*From Professor C. E. Mendenhall, Physical Laboratory, University of Wisconsin.*]

FEBRUARY 27, 1903.

Your letter of February 2 came duly to hand, and I have made a few notes concerning that part of the proposed work with which I am somewhat familiar. These I am very glad to send you now, hoping that the delay has been of no inconvenience to you. I have not thought that you wanted great detail, and have tried to avoid it. Though most if not all of the suggestions are such as must have already occurred to you, I nevertheless give them, thinking that perhaps they may usefully serve to confirm if not to initiate.

As regards the general scope of the work, I shall not presume to speak further than to emphasize the importance of one point which you mention, namely, provision for the study of such laboratory problems as seem intimately connected with the solar and stellar work.

In deciding upon the site it may be well to keep in mind, besides the primary requisites, the fact that the observatory will demand a constant though small supply of power. Possibly in California some long-distance transmission line or water power could be drawn upon.

For the solar constant work examples of all the best forms of pyrheliometers and actinometers should be provided and studied with a view to improvement. No one of them seems independently reliable at present, though the Ångström compensation pyrheliometer promises best.

For detailed infra-red spectroscopic work there is no doubt that the bolometer is the most immediately applicable, because of the work of Abbot and others at the Smithsonian Astrophysical Observatory. But the radiometer, thermopile, and radiomicrometer each has its peculiar advantages, so that if they were properly modified for linear spectroscopic work they might be used with advantage at the two substations. It seems to me unlikely that they will ever supplant the bolometer for accurate linear work in cases where the best must be had at any cost.

In designing the spectrobolographic outfit it seems to me questionable whether it should be as large as that of the Smithsonian Institution. At any rate, even if one outfit of such size is provided for the main station, it would be desirable to provide another of considerably smaller size, more or less self contained and capable of being used with advantage by a single observer. For some of the work

this could be used in place of the larger outfit with resulting saving in time and labor, and for much of the preliminary work and for the laboratory investigations it would be decidedly more convenient.

A storage battery equipment will be a necessity, small and portable, for the substations, but of considerable capacity for the main observatory. Especially should this be the case if laboratory work is undertaken, as this will undoubtedly involve the production of high temperatures which can best be done electrically. In this case also accurate means of measuring high temperatures will be necessary and a variety of laboratory sources of radiation should be available.

Of course, work would be greatly facilitated by a generous assortment of miscellaneous laboratory apparatus, such as small spectroscopes, telescopes, mirrors, polarizing apparatus, air-pumps, etc., and a lot of laboratory supports and attachments. Finally, as much of a shop equipment as is possible.

In connection with the spectrobolographic work, I must confess that at present the further detailed study of the infra-red solar lines does not seem to promise very much. Undoubtedly the true solar lines can be separated from those of terrestrial origin and identified to a greater or less extent with emissions of known elements, and this would perhaps be of most value in connection with the question of the persistence of given emissions through long ranges of temperature. Again, if the sun's surface were studied in greater detail the infra-red lines might help in the study of motions in the solar atmosphere, but it does not seem likely that they would be more important for that purpose than the lines photographically observable. Of course, it *may* be found that the infra-red lines behave in some ways quite differently from those of shorter wave length, and hence furnish a valuable tool for solar investigation, but it does not seem to me that the work so far done leads one to expect this. However, you know much better than I what to expect. Again, is it not true that the solar constant work and the separation of true solar lines from those of terrestrial origin are the parts of the work which really demand the high and low mountain stations, while much, if not all, of the other work, on account of its more intimate connection with laboratory experimentation, could better be carried on at a more centrally located observatory; for example, the Yerkes, best of any.

[*From Professor A. Ricco, Director of the Royal Observatories of Catania and Etna, Sicily.*]

[Translation.]

FEBRUARY 28, 1903.

With regard to the project under consideration by the Committee of the Carnegie Institution, of which you are a member, it is certain that if a station at the summit of a very high mountain is needed for the study of the solar radiation, another station will also be required which may be conducted without encountering the difficulties which are unavoidable at elevated stations; among others, the frequency of days when the mountain is enveloped with clouds due to the condensation which it produces. Etna, for example, as seen from Catania, is enveloped with clouds 167 days in the year, on an average.

The other station should not be placed at the foot of the mountain, since it would also experience the effect of condensations caused by the mountain. At Catania the clouds cover on an average only 39 per cent of the sky (Palermo, 46 per cent), but this amount of cloudiness would be still smaller if it were not for Etna, since the clouds appear more often on the side toward Etna (north) than elsewhere. Furthermore, the station at the base of the mountain would not have an entirely free horizon in one direction.

There should also be a third station on a very extensive high plateau on a small island, in order to have a very homogeneous atmosphere in perfect equilibrium, and to avoid disturbances of the images caused by ascending currents; these are very pronounced during the hotter part of the day on Etna, and carry up with them visible vapors, which frequently hide the sun, and invisible vapors which produce remarkable absorption of the solar radiations. On Etna the curves of heat received by the Arago actinometer have this form [figure not reproduced here]. This may be explained by the absorbing action of the ascending vapors during the hours of greatest heat. At Catania the curves have the regular form, rising with the altitude of the sun.

At Catania, as at Palermo, the images of the sun are best in the early morning; ordinarily during the hot seasons they are bad at 10 o'clock; they become better again before sunset.

[*From Professor A. Belopolsky, Imperial Observatory, Pulkova, Russia.*]

[Translation.]

MARCH 11, 1903.

The project of constructing several astrophysical observatories in the United States is of very great importance. Investigations of the sun are precisely those which require study by modern methods. At the present time, since the discovery which you have made with the spectroheliograph, the study of the solar surface with instruments of sufficient size promises to reconstruct current ideas regarding the constitution of the sun. The glory of accomplishing this will belong to the United States if the construction of special observatories is provided for.

It will perhaps be possible to find a mountain more advantageous than Mount Whitney for investigations of the solar radiation. I believe that the station can be established only at a distance of 300 meters below the summit. The plan for the investigations on the constitution and radial motion of stars also requires instruments more powerful than those which are employed at the present time. Everyone desires to undertake such work, but no country can realize this desire, since nowhere are sufficient funds available for scientific researches. It is only in the United States that private fortunes are devoted to science.

But, as you are well aware, successful investigators are quite as necessary as instruments. I believe that as many scientific investigators as the newly established observatories will need may also be found in America.

[*From Professor Cleveland Abbe, U. S. Weather Bureau, Washington.*]

APRIL 3, 1903.

Your letter of March 25, as Secretary of the Commission relative to the establishment of a large astrophysical observatory, interests me very much. The practical question as to the location and maintenance demands first consideration. There is no doubt that an observatory at the highest practicable point, working in cooperation with one lower down, will eventually add much to our knowledge of the solar and the terrestrial atmospheres. I consider Mount Whitney the most desirable summit station; a station at its base is necessary both for supplies and for special work on absorption. Another station in nearly the same meridian but farther south can

scarcely be found, but in place of that I think that the location of the Flagstaff observatory would be an admirable substitute. Among the solar investigations, I hope an effort will be made to get at the differences in radiation from different spots on the sun's surface in their successive rotations, so as to get the chronological variations in temperature, as well as the geographical differences. The complete course of work that you have sketched out covers all the problems that have thus far been found worthy of study, and, of course, the observatory will take up new ones as fast as they develop.

I should not encourage duplicating in the southern hemisphere all the investigations that are to be conducted in the northern, but there are some problems that could advantageously be studied at both observatories. On account of our knowledge of atmospheric conditions at the stations Charcani and El Misti, that location has some advantages, but there is still hope of finding an equally advantageous location on the mountains of Ecuador or southern Colombia. When such a station is found, it should be devoted especially to studies on nebulæ and stars, such as are described in your article 3, and perhaps also to less extensive studies on the determination of the solar radiation constant.

From a meteorological point of view, observations on these northern and southern mountain stations are extremely desirable, and especially if observers at the upper and lower stations can make absolute determinations of the altitudes and motions of the clouds or temperatures of the upper air by means of kite and balloon ascensions. Many other studies into the physics of the atmosphere, such as its gaseous constituents, its dust, and its motions, would constitute valuable additions to our meteorological knowledge. The special field to be occupied by such an observatory relates to the highest attainable atmospheric strata.

If there is any specific matter on which I can be of use to your Committee, I shall always be happy to respond.

[*From Professor G. Müller, Royal Astrophysical Observatory, Potsdam, Germany.*]

[Translation.]

APRIL 4, 1903.

The plan to establish a great astrophysical observatory at a particularly favorable site, and to provide it with the best instrumental equipment, will be greeted everywhere with lively interest. For

many problems of astrophysics the atmospheric conditions experienced at most of our observatories are to be regarded as unfavorable. Every increase in the transparency and also in the steadiness of the air marks an advance, and for this reason the choice of a site for a new astrophysical observatory should be made with the greatest possible care.

The best conditions of the atmosphere are to be expected at a mountain station, and, according to my experience, an isolated peak is to be preferred to a point within a great mountainous region, where surrounding peaks under certain circumstances exercise a strong influence on the state of the atmosphere.

The higher the chosen point the better, but height is not the only important factor. It is far more important that the observatory shall not be too difficult of access, and, before all e\se, it is essentia that the observations can be made with as great convenience andl ease as at any other observatory. Occasional observations, such as are made during a very short period on a very high mountain under the most difficult conditions, are ordinarily of comparatively little value. Many problems of astrophysics, such as the determination of solar radiation and investigations on the absorption of the atmosphere, etc., cannot be solved during short expeditions, lasting days or weeks; they demand systematic study during a long period of time under the most varied atmospheric conditions—if possible, simultaneously from a peak and from a valley station.

In my opinion, it would be best to establish the proposed principal observatory as high as possible, perhaps at an altitude of about 8,000 feet, but in any event so that it could be kept in operation throughout the entire year, or at least through the greater part of it, and be at all times accessible without too great difficulty. At this place the principal instruments should be established, and all investigations conducted which relate to the spectroscopy and the photometry of the fixed stars, particularly those in which photography is employed. An important requirement is the provision of a second *permanent* station at a height of about 1,000 feet. This should likewise be provided with the best instrumental equipment, with the object in view to provide for certain observations which should be made simultaneously at both stations. It is naturally desirable that the direct distance between the two stations should be as small as possible, and that they should be connected with each other by telegraph and telephone.

As regards the program of investigations prepared by your Com-

mittee, I think that this covers all fields of astrophysics in an exhaustive way, and that hardly anything of importance can be added to it. I beg briefly to call attention to a few special investigations which, in my opinion, deserve special consideration:

(1) Investigation of the atmospheric lines of the solar spectrum by simultaneous observations at both stations under the most diverse atmospheric conditions and at all times of the year.

(2) Determination of the extinction of starlight by simultaneous photometric observations at both stations.

(3) Thorough investigation of the photographic extinction by simultaneous photographs of given groups of stars.

(4) Determination of the absorption for various regions of the solar spectrum by spectro-photometric measures.

(5) Comparison of the light of the sun with that of the moon, the planets, and the fixed stars, to determine by continued observations whether any variation in the intensity of sunlight can be detected.

In conclusion, I heartily wish you success in the great undertaking, and trust that your plans will soon develop in the interests of our science.

[*From Professor J. Hartmann, Royal Astrophysical Observatory, Potsdam, Germany.*]

[Translation.]

APRIL 12, 1903.

Let me extend to you my heartiest congratulations on the astonishing advances which astrophysics has already made in America. It must be acknowledged without envy that the new continent has wholly outstripped the old one, and I would regard the establishment of a high altitude observatory as a glory to American science. When, as here in Potsdam, one is forced to observe with a great refractor under very bad atmospheric conditions, one soon comes to appreciate the enormous advantages of a high station with transparent air and quiet images. I will mention here one point in particular regarding which I have had some experience. If a telescope is to be used for spectrographic investigations it is necessary to have the greatest possible aperture in order to secure great light grasping power. If the ordinary ratio, about 1 : 18, of aperture to focal length is employed, this large aperture corresponds to a very great focal length, and in consequence of this the images become so bad with unsteady air that the advantage of the great aperture is almost

wholly lost. In this way, *in an unsteady atmosphere*, our photographic refractor of 32 centimeters aperture gives practically the same results as the great refractor of 80 centimeters aperture. If a greater ratio of aperture to focal length is chosen, say 1 : 10—quite apart from the great thickness of glass required in the refractor—new difficulties result, in that the aperture of the collimator can be only 1 : 10, and consequently for a given size of prism the collimator will be too short.

At a high station these several difficulties are not to be feared. If you find a station with very quiet images, I should recommend for spectrographic work—either with very high dispersion on the brightest stars, *c. g.*, for the determination of the solar parallax, or with smaller dispersion on faint nebulæ and stars—a reflector with great aperture, and a ratio of aperture to focal length of about 1 : 30 or still less. I should also choose such a construction as to permit the spectrograph to remain in a constant position, with the collimator horizontal or in the direction of the earth's axis. Such a spectrograph with long collimator, very short camera, and very high dispersion would be best adapted to carry out the very important investigation on the motions within nebulæ to which I recently called attention. Our apparatus here did not permit me to accomplish much in this direction. If the spectrograph were built in a fixed position, not suspended from the eye end of the telescope tube, it could be made much more stable and also easily maintained at a constant temperature. It would thus be possible to employ very long exposure times. A horizontal mounting is also to be recommended for spectroheliographs of the largest dimensions. It is important to make these photographs with a very large solar image, in order, for example, to be able to study with precision the motions within a sun spot. Spectrographic studies of the zodiacal light and the aurora should also be included in the program.

I heartily wish success to the great undertaking, and it will give me pleasure if I can aid it in any possible way.

[*From E. Walter Maunder, Esq., Royal Observatory, Greenwich.*]

MAY 14, 1903.

With regard to the question of astrophysical research, my own position has led my thoughts in two directions.

To me sun spots seem to be the most important subjects of study. Our work at Greenwich consists, as you well know, in taking two

photographs of the sun daily on a scale of 1 decimeter to the solar radius and measuring one of these for the area and position of the spots. So far I think we fulfill our purpose sufficiently well. A larger scale is not necessary for positions of the accuracy we seek; indeed, the scale of 1 decimeter to the solar diameter was sufficient for that purpose and was less costly and the photographs were more manageable in size. To attempt to push the work of measurement and reduction to a further refinement would immensely increase the cost, and I doubt whether it would repay the outlay and trouble.

But when we come to the question of the details of the spot forms and of their changes, then this scale is certainly not adequate. I greatly wish, and have done so for years, that we had here a second telescope with which we could take comparatively small areas of the solar surface on a scale of at least 1 meter to the solar radius. I think this is needed to supplement the other.

But the study of spot spectra is much more urgently needed. My own slight experience of it with a most hopelessly inadequate instrument was sufficient to make me feel that it was absolutely one of the most important lines of research.

I have always felt it a great misfortune that Sir Norman Lockyer, in the work which he has carried on at South Kensington for so many years, should have devised the method of recording just the "twelve most widened lines." It seems to me in every way a badly devised scheme. If a long series of observations are conducted by one and the same observer, I should think it ran a great chance of stereotyping more or less accidental impressions. If the observer is often changed, we have no longer any means for comparing observations made at different times; and, at best, if we assume the observations free from all personality and absolutely immaculate in quality, they seem to me to tell us hardly anything at all. The general character of any particular spectrum—not to speak of important details—is left absolutely without record in such a system.

Sir Norman Lockyer's chief result, namely, that the most widened lines change with the progress of the spot cycle, opens out a great number of questions. First, the spots at maximum are not only more numerous, but they run much larger than at minimum. It would be most important to observe both at minimum and at maximum a series of spots of a definite size. I would suggest, as it is a size sufficiently frequent even at minimum, a spot of 200 to 300 millionths in area. Clearly it is a great assumption, if we find a

certain spectrum given us by a spot of area 200 at minimum and another spectrum given us by one of 2,000 at maximum, to ascribe the change to a quality in the period (if I may so express it) when it may be a function of the size of the spot itself. Further, the average spot group goes through a certain pretty well defined routine in the course of its growth and decay. Now, if we are at the minimum of the cycle, our groups run small, and it is only (in most instances) during one particular phase of its development that a group is likely to be a tempting object for spectroscopic examination. At maximum we may have plenty of giant groups, which can easily be followed spectroscopically during their whole career. Here, again, is a point which wants to be followed out. If we record a given spectrum for a certain spot, we have not learnt all that we can unless we trace the history of that spot back to its rise and onward to its disappearance, and determine at what particular stage of its development the observation was made.

We want to know whether we can associate different spectroscopic appearances with—

(1) The size of the spot; since the larger spots may be assumed on the average to be the deeper.

(2) The stage of its development. The depth may alter with the age.

(3) The changes that are going on in the group.

(4) The progress of the general solar cycle.

(5) The type of the spot group.

I have put the type last, out of its logical order, because it has seemed to me that, though the great majority of spot groups conform to one general type of evolution, yet occasionally we get spots of a very distinguishable form, and it is these spots, when of immense size, and not spots of the normal type, that are clearly and unmistakably associated with magnetic storms. It would be no small matter if we found that such spots exhibited some distinct spectroscopic peculiarity.

As to the method of observation, clearly the photographic registration of spot spectra should be the routine one; but it certainly should not exclude the direct visual work. Just precisely as our daily photographs of the sun's surface at Greenwich, however admirable for their purpose, leave us without any record of the processes of rapid change, so it would be with the photographic registration of sun spot spectra. They cannot possibly render direct work unnecessary.

Might I quote from a letter to me from Mr. Evershed on this subject. He writes:

"We are apt to become too familiar with sun spots to be surprised at their occurrence; but I am sure that when their origin and meaning is really understood, a key will be found to a great many other solar and stellar problems. I consider that as a preliminary the spot spectrum should be investigated with a bolometer in the visible region and infra-red to find out whether the discrepancy between thermal and visual estimates of spot darkness is real, and, if so, where in the spectrum is the excess of radiation measured thermally.

"Another point to clear up is the resolution of parts of the spot band into lines observed by Young and Dunér. Does this apply to all spots and to all parts of the spectrum, and is the emission spectrum of the photosphere itself really continuous under high dispersion?

"It seems to me that until the fundamental radiation of spots is satisfactorily cleared up the study of widened lines is of secondary importance."

The other subject to which my attention has been directed is the study of Jupiter. It seems to me that that planet ought not to be left to the scrutiny of amateurs, but should be systematically observed at some permanent and endowed observatory. As the sun is the only hot star which we can study in detail, Jupiter is the only cold star, and we are fortunate in having representatives of both ends of the series within our reach. In my connection with the British Astronomical Association, my attention has been drawn to the special field for work which this planet offers. The Association has done what it could in the matter. Its object was the training of amateur observers, and their direction to real systematic purposeful work in place of the desultory star gazing which is too often all that amateurs achieve. So far it has been most successful, and the Jupiter Section numbers quite half a dozen observers of the very first rank, beside others who may in course of time attain the same skill. But an association such as ours can never be a substitute for a permanent observatory. A very large proportion of its efforts must be spent in the work of training; there is no guarantee that any of its observers will be able to follow up a research for a long-continued period, and the means for the proper discussion of observations are quite lacking. I might mention, as an example, one of our most active students of Jupiter, Capt. P. B. Molesworth, R. E., of Ceylon. He has been working there for nearly eight years, and in a single apparition has obtained nearly 4,000 transits of spots

across the central meridian of Jupiter, revealing not a few interesting relations; but necessarily he will not be able to remain much longer at that station, nor can he give to his observations the full discussion they deserve. If work similar to his could be undertaken by a professional astronomer, who would have the time to fully discuss his results, at a permanent observatory, which would secure continuity to the work, I think it would ere long lead to our understanding the condition of Jupiter far better than we can at present. Obviously an equatorial or at least a tropical site for such an observatory would, as Captain Molesworth has found, have great advantages.

[*From Professor Knut Ångström, Royal University, Upsala, Sweden.*]

MAY 16, 1903.

I beg you to excuse my long delay in answering your very interesting communication on the astrophysical observatory planned by the Carnegie Institution, but a great many duties have hitherto made it impossible for me to reply. I hope, however, that you will not see in that delay a proof of indifference regarding a question that in fact interests me profoundly.

Unfortunately I can give no information as to the site of the observatory, having no personal experience with regard to the atmospheric conditions in the mountains of California. The establishment of two corresponding observatories at different heights will certainly be most valuable for the scientific results. As to the choice of a place, it will probably be of great importance to study the local conditions. My experience, derived from visits to Teneriffe, is that on the northern side of the mountain it is almost impossible to get satisfactory results in solar observations, while the southern side is very favorable for that purpose.

As to the instruments for measuring the total radiation of the sun, I am sure that the compensation pyrheliometer is at present the only convenient instrument, and I am willing to superintend the construction of the instruments that the observatory may think proper to order from the mechanician Rose in Upsala.

Probably the program of the observatory comprises also the registration of spectral energy by means of the instrument of Professor Langley. I may, however, call your attention to the advantage of making the registration also with another instrument, with less dispersion, which in a shorter time could give a general view of the

solar spectrum and of its changes. I send you an account of a simple arrangement for that purpose.

It would be of special interest if these observations could be combined with researches on the amount of humidity in the free atmosphere (by means of kites). I hope to return later to certain questions that I believe to be of great importance. It will always be a great pleasure to me if I can be of service to you.

[*From H. F. Newall, Esq., The Observatory, Cambridge, England.*]

CAMBRIDGE, *May 22, 1903.*

I almost hesitate to put down some of the ideas that have occurred to me about the establishment of a large astrophysical observatory, for they are rather crude, incomplete beginnings than formed judgments, and if I put them down at all it is only in the hope that they may help to clear matters rather than with the idea that they can carry any weight.

First of all, let me say it seems a grand project to provide for an observatory for observations of *secular* physical phenomena of solar origin. It is perhaps an open question whether it is desirable to provide for allied stellar studies. Many people must share the same instinctive doubt about a universal observatory as about a universal instrument. One may definitely count on individual enterprise to provide for many of the researches indicated in your paragraph (3). Existing observatories do or can or should deal with most of the studies referred to, and it would be a pity in any way to risk cramping either performance or obligation in these matters. Moreover, there are the elements of competition; as, for instance, in determination of motion in the line of sight for *fainter* stars one may count on advance from existing observatories.

If in your large astrophysical observatory large special apparatus were available it might well be desirable to let the place become, as it were, a court of final appeal, whither perhaps rival pioneers might themselves resort to put their views to the test. In such cases, for instance, it might be a matter of "more light" being needed, and the pioneers might be expected to take with them their own eye end apparatus for attachment to a large light grasping instrument; but one would like to provide in every way against retarding small private enterprise and pioneering elsewhere. One must avoid anything that would lead to the position, "Oh, it is no use doing this or that; they have that on their program up there."

Where natural competition is absent or periods of phenomena are long, there is a grand opening for a powerful combined attack. One may count, for instance, on special studies of solar rotation at existing observatories, simply because the period is short, but probably the variation of such short period phenomena in the eleven year period could only be properly tackled in an observatory where the outsetting aim was the study of secular phenomena.

Hence it seems to me that secular phenomena are the special province of such an observatory as you are contemplating; and it would seem a wiser course to concentrate attention on such observations as would have direct bearing on these, and to provide for a systematic discussion of observations already accumulated, as well as for a systematic study of phenomena in process of being observed, than to scatter forces on the study of many stellar problems.

In many ways probably more advance could be made by enabling a single observer to carry out his observations in several stations successively. The solar radiation "constant" is an instance in point. Considerations of this sort would lead me to think that in some ways it might be better policy not to lock up huge capital in one fine observatory outfit, but rather to help individual researches by providing means for having them carried on with, say, one or two complete outfits that could be moved to various points of the globe.

As I say, I hesitate to commit these remarks to paper. I suspect you are far beyond the elementary stage that these remarks refer to.

As to aims and researches, your program is a large one already. It is not clear to me why it should not include a new attack on magnetic disturbances, and possibly on atmospheric and electrical phenomena.

[*From Dr. Ralph Copeland, Astronomer Royal, Royal Observatory, Edinburgh.*]

AUGUST 15, 1903.

I am afraid you will think me remiss in only now replying to your letter of March 26; but I have indeed most carefully thought over your project and looked up my old papers on mountain observatories. I have not much to add to the views which I expressed in my paper on the subject in volume III of *Copernicus*, which you have; but, when consulting it, kindly substitute on page 230, line 22, 1.32 inch for 0.7 inch.

Another note on my South American trip, written for the British

Association Report for 1883, may interest you, and I therefore inclose a couple of copies.

My own impression is that to reap the full benefit of a mountain station one should aim at a height of fully 11,000 feet, and if well within the tropics an elevation of 12,500 feet might be occupied throughout the year withont serious discomfort. Such an altitude in either temperate zone, however, would expose the observers to the most terrible weather and great hardships in the winter—*e. g.*, the floor of the Crater of Elevation of Teneriffe (7,200 feet, latitude 28¼°), according to the late Professor C. Piazzi Smyth, is swept by violent snowstorms every winter. The experience of residents and travelers in your own mountains will furnish you with abundant further examples.

My experience at Puno on Lake Titicaca (12,500 feet), in latitude 15° 50′ south, proves that observations can there be carried on under favorable conditions of weather and temperature at all seasons of the year. Indeed, the sky, on the whole, is much clearer in the winter, and therefore better suited for observations in general, although there are doubtless certain solar investigations which could in that latitude be better prosecuted at a season when the sun passes within a few degrees of the zenith for many weeks in succession. In the months of October, November, and December the weather is often very fine, I was told; but in January, February, and the early part of March clouds, and even a good deal of rain, are to be expected.

As you will know from *Copernicus*, my experience in the Andes was confined to the neighborhood of the Mollendo-Puno route, where, through the courtesy of the railway authorities, mechanical and technical aid is readily procurable; but doubtless the same mechanical facilities would be offered on the Oroya railway, which, starting from Lima, in 12° south latitude, reaches a height of fully 15,600 feet quite near the Pacific seaboard. Unfortunately the disturbed political state of the country at the time of my visit prevented me from examining this railway, but from its position so near the rainless coast it is very possible that the weather conditions near the upper part of the route may be fully more favorable than on the Mollendo railway.

But I should here like to draw your attention to a point affecting the personal comfort and even safety of the members of an astronomical party on their way to a high-level station. In my opinion, the whole of the ascent should not be attempted on one day; the party ought to devote something like a week to inuring themselves

to an elevation of 7,000 to 10,000 feet before proceeding to the more trying height of 14,000 or 15,000 feet. In my own case, very much against my will at the time, I was detained at Arequipa (7,750) feet for a whole week, with the advantageous result that I experienced hardly any inconvenience when from there I went on to Vincocaya, at a height of 14,360 feet. I mention this in particular, as such very unfavorable reports are current regarding the railway journey on the Oroya line from Lima and the mortality among the workmen employed in constructing that very remarkable railway.

As regards the desirable instrumental equipment, there is one point which I desire to emphasize. The mirrors of the reflecting instruments should be made of speculum metal and by no means of silvered glass. Polished silver is incomparably more liable to tarnish than good speculum metal. Besides, even what would be called a good film of silver on glass is in a considerable degree transparent to ultra-violet rays, as was pointed out by Stokes and Cornu many years ago (*Annales de l'École Normale Supérieure*, ser. II, tome IX, 1880, pp. 22–23). Respecting the behavior of such a film with regard to the infra-red rays I have no knowledge, but doubtless your own Professor Langley has had abundant experience on this point. On the other hand, mirrors of speculum metal reflect the low grade heat rays of the moon, and all other rays up to the extreme known limits of the ultra-violet, with apparent equal completeness. Moreover, a mirror of speculum metal, when made of the proper alloy and well polished, is, under proper care, one of the most permanent of known optical appliances. I have before me the Cassegrain mirror of a reflecting telescope of 6 inches aperture, made by Short in 1745; both it and the other mirrors of that telescope are, to use the words of Dr. Dreyer, "as bright as if they had been polished yesterday." I must add, however, that the telescope, which formerly belonged to King George III, and is now at the Armagh Observatory, seems to have been but rarely used. We have here, however, a Gregorian reflector made by Cary something over 100 years ago, which is frequently used by us for watching the timeball, and though never repolished, is still so bright that one would hesitate to relegate it to the polisher. We have also the magnificent 5½-inch grating presented by the late Professor Rowland more than 20 years ago, also made of speculum metal, which is practically as good as when it was first received by Lord Crawford, though it has been in very frequent use. During one course of observations this grating was exposed to the fumes from peaty ground, which we

found very injurious to silver on glass, but which did not perceptibly affect the brilliancy of the surface of the grating. Doubtless you will be able to learn the exact composition of the alloy used for the Rowland gratings; but probably the combination of 4 atoms of copper with 1 atom of tin, recommended by the late Lord Rosse in his account of the construction of the great telescope at Birr Castle, would yield an alloy capable of retaining its polish for many years. From Lord Rosse's own account it seems that he himself used a somewhat softer alloy, with the consequence that the larger mirrors made by him required repolishing about once every two years; but I may mention that the night air at Birr Castle Observatory is usually very damp, and that owing to the great mass of the mirrors they are very liable to get dewed. This brings me to another part of the subject.

A serious objection to metallic specula, as usually constructed, is their great weight; but this difficulty may be largely remedied by giving the metal a more suitable form than that of a simple circular slab or disc. By the use of suitable sand (such as is used in the production of the highest class of bronze castings) there should be no difficulty in casting a speculum with deep ribs on the back, which would be much lighter and relatively stiffer than a disc of the same diameter. For a mirror 54 inches in diameter, I would suggest making the ribs and face of a uniform thickness of ½ inch. By giving the speculum a total depth of 8 inches, it would probably be stiffer than any mirror yet cast, and with a suitable arrangement of the ribs would weigh about 1,200 pounds. Of course, I assume that the "metal" would be cast face downwards on a "bed of hoops" of the proper curvature, as practiced by the late Lord Rosse, to insure that the surface to be ground and polished should be perfectly sound and almost exactly of the desired form. The possibility of casting a speculum of this shape was, in a great measure, set at rest by an experiment made by the present Lord Rosse, who cast an elliptical flat mirror some 11 inches by 8 inches with a ribbed back, some thirty years ago. This mirror was perfectly sound and took a very high polish. It was used as a diagonal mirror for the 6-foot Newtonian reflector. If I were trying the experiment I should be inclined to honeycomb the mirror after this fashion, adding a "web" round the outside, but taking care to make every part, including the face of the mirror, of the same thickness to facilitate uniform contraction in cooling. It is almost needless to say that a casting of this kind would need to be carefully annealed. Provided the

pattern were made in two parts, back and front, of cast iron and carefully finished, it is quite possible that a uniform thickness of 3/8 inch would be quite sufficient; this would reduce the weight of the finished speculum to 900 pounds. Probably the chief practical difficulty in making such a casting will be so to arrange the mold that it will readily yield to the contracting speculum metal. Very much will depend on the nature and condition of the sand or loam used in forming the mold, but doubtless valuable advice on the whole process could be obtained from an experienced molder who has been accustomed to the production of complicated and fragile castings.

In conclusion, if we regard the rapid progress in spectroscopy of late years associated with the improvement of the diffraction grating, it seems that this is probably no less due to the happy choice of speculum metal for the material of gratings than to the improved accuracy of the ruling. It is therefore reasonable that further advances in many other branches of astrophysics may be expected from a return to the use of the solid metallic reflector in place of the mirrors of silvered glass now so much in favor.

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